

ABSTRACT

Title of Thesis: The Deterrent Effects of Police Patrol Presence On
Criminal and Disorderly Behavior at High Crime Locations

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This paper tests the deterrent (or displacement) effects of preventive patrol upon criminal and non-criminal disorderly behaviors at high-crime locations ("hot spots") using observational data collected during a preventive patrol experiment in Minneapolis from December 1988 to November 1989. The analyses reveal that the immediate presence of uniformed police directly reduces the outbreak of disorderly conduct at hot spots, but this effect is contingent upon raising the overall level of proactive presences at hot spots. Increasing patrol levels at hot spots also produces residual deterrence which decreases disorder during times when police are not present at these locations. Such residual decreases in disorder are larger than the direct deterrent effects of police presence when patrol is at normal levels. Further, direct and residual deterrence generated by patrol are stronger for criminal acts than for a combined measure of criminal and non-criminal disorderly behaviors.

The analyses employed survival models to estimate the effects of specific instances of patrol presence upon the time to the first disorder (criminal or non-criminal) after police depart from a hot spot. Using presences up to 20 minutes in length, these models reveal that longer presences increase survival time, thus enhancing residual deterrence. However, there is evidence this effect decreases after presences pass about 14 minutes in duration. Moreover, stops must be about 10 minutes in length in order to produce significantly better survival times than those produced by driving through a hot spot. The theoretical and policy implications of these results are discussed.

THE DETERRENT EFFECTS OF POLICE PATROL PRESENCE
ON CRIMINAL AND DISORDERLY BEHAVIOR AT
HIGH CRIME LOCATIONS

by

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DEDICATION

To my parents, John and Linda, and my brother, Chad

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INTRODUCTION

This paper tests several hypotheses about the deterrent or displacement effects of police patrol presence upon disorderly and criminal behavior at high crime locations. In doing so, it draws upon theory and research regarding the general deterrent effects of police patrol, the debilitating effects of social disorder, police responses to social disorder, and strategic targeting of specific crime-prone locations ("hot spots"). The observational data used to test the hypotheses were collected as part of a one year preventive patrol experiment in Minneapolis conducted from December 1988 through November 1989. These data provide a unique opportunity to study the interaction between visible police presence and disorderly and criminal behavior. The analyses test the claims that there is less disorderly and criminal behavior when police are present, that increased patrol levels produce residual deterrence which decreases disorder and crime when police are not present, and that, with regard to specific instances of police presence, stronger dosages create residual deterrent effects. The last hypothesis is tested using event history analysis.

I. POLICE PATROL AND DETERRENCE

DETERRENCE THEORY AND PATROL

Deterring criminal conduct through legal threats is a fundamental aspect of crime control efforts. Deterrence scholars indicate certainty of punishment is generally thought to have greater deterrent value than severity of punishment (Andenaes, 1974; Cook, 1980; Zimring and Hawkins, 1973). As an obvious, visible indicator of sanction threat, police patrol occupies a central place in crime control strategies. Yet during the past two decades, researchers have questioned the deterrent value of patrol. This paper attempts to reveal more insight into the effects of patrol presence upon behavior.

Patrol presence is a visible threat which increases the public's objective and subjective certainties of punishment. Zimring and Hawkins (1973), for example, discuss the role of law enforcement in making legal threats credible. They state credibility is a matter of cues, such as visible police presence, and how those cues are interpreted (1973: 171). They further write that direct experience and word of mouth knowledge of law enforcement presence and actions are factors affecting perceptions of credibility (1973: 163). Likewise, Cook (1980: 223-224) states that visible police presence increases certainty of

detection and apprehension, and that frequent police presence in an area can increase potential offenders' perceptions of risk in that area. In addition, he states that even when public perceptions of threat are inaccurate, they may still be systematically related to criminal justice activities (1980: 222). If so, increases or decreases in the level of police presence should raise or lower, respectively, perceptions of threat even though those perceptions may overstate or understate the true level of risk.

The strategy of preventive patrol is to have officers randomly drive or walk through an area when not answering calls for service, thereby creating uncertainty for would-be offenders. Kelling (1985) states preventive patrol is intended to create a feeling of police omnipresence. Thus, potential offenders should be deterred even when police are not within visual range because one never knows when an officer will appear.

Notwithstanding these points, Zimring and Hawkins (1973: 171) speculate there is a point at which further increases in patrol levels may yield little additional benefits. Moreover, they question whether raising the level of police presence will, in and of itself, increase deterrence if the increase is not enough to actually improve the objective probability of detection (1973: 164). Considering that studies of police in Kansas City, Missouri and Lansing, Michigan indicate few officers spend their patrol time attentively watching for problems, increases in patrol may not necessarily lead to substantial increases

in the certainty of punishment (Sherman, 1983: 151).

On the other hand, Zimring and Hawkins (1973: 168-169) also present evidence indicating police visibility can have a deterrent effect on some forms of offending even when the objective probability of apprehension declines. They report that during a seven month period in 1967 Detroit police only issued about half as many traffic tickets as normal with no apparent ill effects.¹ Using the accident rate as an index for the real number of traffic offenses, Zimring and Hawkins found the accident rate actually declined. Throughout this period, the number of officers on the roads had not changed. This suggests visual cues have an independent role in deterrence. Although the actual probability of punishment had decreased, the visual cues of enforcement remained unchanged.

Overall, however, the issue is still unclear. Indeed, Zimring (1978: 164), commenting on the general lack of theoretical structure in deterrence studies, writes there is no clear relationship between variations in car patrol and perceived certainty of punishment.

BASELINE EFFECTS OF POLICE

As Cook (1980: 214) points out, debates regarding the deterrent value of

¹Officers took this action as a result of dissatisfaction over working conditions and wages.

criminal justice policies are centered on the marginal effects of changes in the certainty or severity of punishment. Apart from these marginal changes, the criminal justice system has a deterrent effect of unknown magnitude.

Dismantling the system would lead to massive increases in crime, according to Cook. Andenaes (1974: 16) concurs, stating, "...a modern industrial society can hardly be kept going without police and penal courts."

Focusing on the role of police, the "baseline" effect of police presence is illustrated by several historical examples. Andenaes (1974) discusses the arrest of the entire Danish police force by their German occupiers in September 1944 and the tenfold increases in robbery and larceny which followed in the city of Copenhagen. Further examples are the massive outbreaks of violence and lawlessness which occurred in response to dramatic reductions in police manpower during police strikes in Boston (Russell, 1975) and a number of English cities (Sellwood, 1978) in 1919. More recently, a 15 hour police strike in Montreal during 1969 led to riots, arson, gun battles, and other disorders, resulting in 3 deaths and more than 1 million pounds in damage (Sellwood, 1978: 209-210).

Hence, it is apparent the existence of police forces has a threshold deterrent effect upon behavior, and this effect is tied to the knowledge that police are patrolling the streets. Yet, there are also examples of police presence failing to deter even serious crime. In Washington, D.C., for

example, increasing numbers of police officers and foot patrols have failed to bring down homicide rates fueled by drug-related violence. In some cases, homicides have taken place within a block of officers and even within visual distance of officers (Castaneda, 1990).

Many researchers and practitioners now question the value of preventive patrol, the traditional means by which police have sought to be visible and deter crime. Increasingly, police departments are seeking alternative ways to use their personnel. This is due primarily to a number of patrol studies which, taken together, suggest only impractically large increases in patrol can create significant reductions in crime.

EMPIRICAL STUDIES OF PATROL AND DETERRENCE

Over the last four decades, there have been a number of studies on the effectiveness of preventive patrol. The first of these evaluated a project called Operation 25 which more than doubled the number of patrolmen in one precinct of New York City during a four month period in 1954 (Wilson, 1983: 62-63).² Compared to the same time period from the prior year, reported crime figures showed reductions in serious crimes, particularly outdoor crimes like muggings and auto theft. However, a number of

²Most of the additional officers were placed on foot posts.

methodological problems with the study make it difficult to draw inferences from the results. For instance, there was no control group, tests for displacement were not conducted, and the study did not properly control for time trends which may have affected crime levels.

A similar effort was implemented in the 20th precinct of New York City in October 1966 when the number of officers patrolling that precinct was increased by 40 percent for an eight month period (Chaiken, 1978; Gallagher, 1978; Wilson, 1983).³ This study improved upon the Operation 25 evaluation by using similar precincts as control groups, examining adjacent precincts for displacement effects, and using crime data which spanned from the beginning of 1963 to the end of 1967. The evaluation found that different types of outdoor crimes showed net reductions. That is, these crimes increased less in the project area than in the other areas. Indoor crimes were not significantly affected. In addition, there was no strong evidence of displacement to adjoining precincts. Nonetheless, the project and control areas were not carefully matched on sociodemographic characteristics (Wilson, 1983: 64), and there were questions about the extent of displacement effects in one of the three adjacent precincts which were examined for such effects (Gallagher, 1978: 178).

Another saturation patrol study was conducted by Schnelle, Kirchner,

³The evaluation of this project was carried out by S. J. Press (1971) for the New York City Rand Institute.

Casey, Usselton, and McNees (1977) in Nashville. Four patrol zones were given extra patrol dosage, two during daytime hours and the other two during nighttime hours. The researchers reported overall patrol movement was raised to 4 times its normal level (as opposed to only increasing the number of officers), and patrol movement under 20 mph was increased to about 30 times its normal level. Time series analysis showed Part I crimes were reduced during the saturation period for those zones having the night patrols only. Analysis of crime in the surrounding patrol zones showed no evidence of displacement effects. After the saturation period, crime levels in the night patrol zones promptly rose to their previous levels. The zones receiving the daytime patrols showed no changes during or after the intervention. However, the treatment periods were very brief (each patrol increase lasted only 10 to 15 days), and the time series analysis only covered a 4 month period. Also, there were no matched control areas.

Two methodological problems shared by the above studies concern their use of officially reported crime (which understates true crime rates) and the relatively brief interventions they evaluated. A study which improved on these weaknesses examined subway robberies in New York City (Chaiken, 1978). This study evaluated six years of enhanced subway patrols and constitutes the longest evaluation of an increase in police presence. Further, it seems likely that crime was reported more accurately in this study because

reporting was more convenient for victims (they could simply report an offense at the next stop) and because many victims were subway employees (Wilson, 1983: 65).

In 1965, special patrols were implemented in the subways from 8 p.m. to 4 a.m. The number of officers in the subways was increased from 1200 to over 3100 with the objective of having an officer on every train and at every station during these hours. The study evaluated the impact of these patrols by examining subway crime, particularly robberies, from 1963 through 1971. Both overall crime and robberies decreased temporarily after the patrols began, but robberies continued to increase at the same annual rate. After two years, overall crime also began to increase at the same rate it had prior to the intervention. There was a lasting deterrent effect that continued through the end of the evaluation period, however, on nighttime robberies.⁴ Thus, the continued increases in robberies were due primarily to increases in daytime robberies, leading Chaiken (1978) to conclude there was displacement of nighttime robberies to daytime hours.

Though most of the studies reviewed above suffer from a number of

⁴The original evaluation was conducted by Chaiken, Lawless, and Stevenson (1974). After the evaluation, there were revelations that transit police had manipulated crime figures in order to make the patrols appear more successful. After reexamining the data, the authors concluded that a deterrent effect still existed though its magnitude was distorted (Chaiken, 1978). The findings discussed in the text are the corrected results presented by Chaiken (1978).

shortcomings, the overall results suggest large increases in police presence decrease certain types of crimes. Yet, increases like those described above are impractical to implement over large areas. This limits the utility of the results. The findings from the subway study are particularly difficult to generalize because subways are enclosed areas with few exits (Wilson, 1983: 66). In the Nashville study, Schnelle and his colleagues did not recommend continued use of saturation patrols due to the costs of the patrols and the failure of even the night patrol areas to show a residual effect after the treatment period.

The most influential study on the effects of police patrol has been the Kansas City Preventive Patrol Experiment (Kelling, Pate, Dieckman, and Brown, 1974). This project randomly assigned groups of patrol beats matched on sociodemographic characteristics to three groups which received varying levels of patrol over a one year period. Altogether, fifteen beats were used. Five of these beats had no changes in their levels of patrol. Another group of five beats was given two to three times the normal level of preventive patrol. The final group of beats had no preventive patrol - patrol cars were to enter these beats only when responding to calls. The evaluation used reported crimes and citizen surveys (administered both before and after the experiment) which questioned residents about victimization, fear of crime, and attitudes toward the police. At the end of the experiment, the evaluators

found no significant differences between the three groups in reported crime, victimization rates, citizens' fear levels, or citizens' satisfaction with police.

The Kansas City study was a breakthrough achievement in criminological research, but its results have been questioned. Larson (1975) has questioned whether the different beat groups really received different dosage levels.

Another difficulty is that the small number of beats in the study and the low baseline rate of crime within those beats created a statistical bias toward the null hypothesis of no difference between groups (Sherman, 1986).

Nonetheless, the study has been very influential, leading many police practitioners and scholars to conclude that preventive patrol has little deterrent value.

Sherman and Weisburd (1988; 1990) have recently reopened the issue with a preventive patrol study in Minneapolis which raised patrol levels at high crime addresses and intersections. Targeting specific high crime locations rather than patrol beats allowed them to use larger sample sizes with units having higher baseline rates of crime. This allowed them to overcome the statistical problems plaguing the Kansas City study. After a one year period, Sherman and Weisburd (1990) found increases in police presence led to decreases in crime as measured by 911 calls. This study will be discussed in further detail below.

Some other studies which should be mentioned here are the foot patrol

studies in Newark, New Jersey (Police Foundation, 1981; Pate, 1986), Flint, Michigan (Trojanowicz et al., 1983; Trojanowicz, 1986), and Boston (Bowers and Hirsch, 1987) which attempted to decrease crime, in part, by increasing police visibility. The Flint study is the only one of the three which claimed to show decreases in crime, but methodological problems with the Flint study, such as having no control group and no tests for the statistical significance of the crime decreases, cloud the findings.⁵ The Newark study involved small sample sizes, thus biasing the crime analyses toward the null hypothesis of no effects (Sherman and Weisburd, 1987; Greene and Taylor, 1988). There is also evidence officers were not always walking and performing their functions properly (Sherman and Weisburd, 1987: 5), and surveys of residents revealed inconsistent findings regarding awareness of foot patrol. This raises questions about the visibility (i.e., dosage) of the Newark treatment. Boston's Patrol Reallocation Plan, implemented in March 1983, deployed 300 foot officers on 98 patrol beats throughout Boston (Bowers and Hirsch, 1987). Nevertheless, there are questions about dosage in this study as well. The beats varied widely in design and staffing, suggesting officers may have been much more visible on some beats than on others. Moreover, some of the experimental beats received relatively little foot patrol (to illustrate, some had a daily

⁵Also, the foot patrol effort in Flint appeared to place more emphasis on community-oriented activities than on patrol visibility. Consequently, the crime reductions (if real) may have been due to the content of policing implemented by the foot patrol officers.

average of only one hour of foot patrol on the day and/or evening shift).

Overall, the literature suggests the increases in patrol visibility needed to decrease crime can only be realistically implemented within small geographic areas. Based on the available research, the most effective and practical avenue for enhancing the deterrent effects of preventive patrol appears to be the location-oriented approach tested by Sherman and Weisburd (1988; 1990). At the same time, it is not entirely clear to what extent this approach and other saturation approaches displace crime or how long their effects can be maintained.

II. THE DISORDER PROBLEM

DISORDER, FEAR, AND CRIME

The aforementioned studies constitute the bulk of knowledge regarding the impact of patrol on crime. Another issue of growing concern is the police response to disorderly behavior and conditions. Evidence from studies conducted in several cities demonstrates that minor crime and otherwise disorderly behavior (such as vagrancy, panhandling, vandalism, public drunkenness, drug use, verbal harassment, prostitution, etc.) and physical signs of decay and disorder (such as broken windows, graffiti, abandoned houses, etc.)⁶ make people more fearful of crime (Lewis and Maxfield, 1980; Lewis and Salem, 1986; Pate, Wycoff, Skogan, and Sherman, 1986; Skogan and Maxfield, 1981; Skogan, 1990; Wilson, 1968). Based on survey results in Chicago, Lewis and Maxfield (1980) argue that disorder has a greater influence on citizens' perceptions than do actual crime rates because citizens witness and experience disorder more often than serious crime. Disorderly behaviors, in particular, seem to engender fear (Skogan, 1990: 47; Lewis and

⁶Skogan (1986: 212) reports that the conceptual distinction between social and physical disorder was made by Hunter (1978).

Maxfield, 1980: 182).⁷ Referring to disorderly behaviors (social disorder) as "soft crime," Reiss (1985: 8) observes, "It is the visibility, frequency, chronicity, and cumulative nature of soft crime that is consequential." This problem has been compounded by the decriminalization of many disorderly behaviors and the deinstitutionalization of the mentally ill (Skogan, 1990: 183).

In addition to creating fear, some scholars argue disorder contributes to more serious crime. In an influential article entitled "Broken Windows," Wilson and Kelling (1982) argue that disorder erodes control over a community and makes it vulnerable to criminal invasion. Their argument is that signs of social and physical disorder make residents and workers fearful. As a result, they isolate themselves and avoid contact with others. This causes informal social control to break down and raises the level of anonymity in a neighborhood. If signs of disorder go unchecked, they become cues for potential offenders, indicating a lack of control over the area. Offenders are attracted to such unruly, anonymous environments. Disorderly behavior and minor violations will then increase, eventually escalating into more serious criminal behavior. Wilson and Kelling feel this process has a degenerative

⁷Available evidence suggests disorder can contribute to fear levels which are higher than would be expected based on crime rates alone (Skogan, 1986). In some cases, the reverse effect can occur. Lewis and Maxfield (1980: 185) observe, "The role of objective crime rates is mediated by perceptions of neighborhood incivility. If incivility is not perceived to be a problem...then it appears that residents can cope with higher crime rates."

effect on neighborhoods.⁸ Citing research on vandalism as supporting evidence of their contagion theory, they conclude a broken window left untended will lead to more broken windows. Accordingly, Wilson and Kelling recommend that police place more emphasis on helping communities maintain order.

Greene and Taylor (1988) criticize Wilson and Kelling's hypothesized link between disorder and crime, stating that research conducted in Baltimore suggests the link between disorder and crime is a spurious relationship attributable to social class.⁹ On the other hand, Skogan's (1990) analysis of survey data from 40 neighborhoods in Chicago, Houston, San Francisco, Philadelphia, Atlanta, and Newark supports the link between disorder and crime.¹⁰ In 20 of these neighborhoods, residents were asked comparable questions about perceived crime problems. Disorder had a strong and significant correlation with crime problems even after controlling for poverty,

⁸In addition to eroding informal social control and the moral authority of a neighborhood, disorder can have a negative impact on the economic vitality of a neighborhood by, for instance, driving away residents with greater financial resources and making the area unattractive to workers and shoppers (Skogan, 1986; 1990).

⁹Greene and Taylor (1988: 201-203) also raise criticisms about the proposed link between disorder and informal social control. Skogan's (1990) research, in contrast, supports the link between disorder and decreasing neighborhood control.

¹⁰Skogan's project integrates data from several studies.

stability, and racial composition.¹¹ In fact, the social and economic factors were insignificant when disorder was taken into account. Moreover, using robbery victimization data collected in 30 neighborhoods, Skogan creates a path model showing that economic and social factors have only indirect links to crime through disorder. Thus, he states (1990: 75):

The evidence suggests that poverty, instability, and the racial composition of neighborhoods are strongly linked to area crime, but a substantial portion of that linkage is through disorder: their link to area crime virtually disappears when disorder is brought into the picture. This too is consistent with Wilson and Kelling's original proposition, and further evidence that direct action against disorder could have substantial payoffs.

THE POLICE RESPONSE TO DISORDER

Police of the 1800s and early 1900s assumed more responsibility for controlling disorderly behavior than do police today. Describing police forces of this period, Moore and Kelling (1983: 53) comment, "The scope of police responsibilities remained very broad: they were responsible for discouraging lesser forms of public disorder (e.g., drunkenness, vandalism, obscenities, harassment, lewdness), for regulating economic activity (e.g., enforcing traffic laws, coping with unlicensed peddlers, inspecting facilities), and for handling everyday medical and social emergencies (e.g., traffic accidents, fires, lost

¹¹Skogan's disorder scale combines measures of social and physical disorder.

children)."¹² Officers made large numbers of arrests for public order offenses such as public drunkenness and fighting (Sherman, 1990: 5).

Reforms of the early twentieth century changed the character of police work. Police forces became more professionalized and adopted crime-fighting as their primary stated function. Car patrols, instituted to decrease response times and improve supervision of officers, isolated police from street life. The growth of 911 systems in the 1970s led to even more emphasis on responses to emergency calls (Sherman, 1986). Consequently, organizational rewards are primarily linked to making arrests for serious crimes and responding to emergency calls.

Furthermore, court decisions and statutory changes have decriminalized behaviors like public drunkenness and made it more difficult for police to get troublesome persons off the street with charges such as suspicion and vagrancy (Skogan, 1990: 87; Skolnick and Bayley, 1986: 139-140). In addition, prosecutors and judges usually give low priority to disorder offenses that are still prohibited. As a result, police find few incentives in the enforcement of

¹²Scholars such as Kelling, Wilson, and Moore feel police forces of this time period were more conscious of protecting neighborhoods and serving neighborhood needs. Walker (1984) critiques this view of early police work. Although police, as agents of local political machines, conducted a number of social welfare tasks, Walker (1984: 87) claims there is no evidence police consciously sought to fight crime and serve neighborhood needs. Further, he states police often lacked political legitimacy in their neighborhoods.

disorderly conduct laws and ordinances.¹³

As part of the movement toward community oriented policing, however, many police departments are attempting to be more responsive to what citizens define as community problems. Knowledge acquired from community policing projects in cities like Houston (Pate et al., 1986; Skogan and Wycoff, 1986) and New York (Weisburd and McElroy, 1988) reveals that residents want police to address disorder problems. Improved communications with citizens and knowledge gained from research have prompted many departments to place greater priority on disorder problems. Police departments in cities such as Denver, Santa Ana, and Oakland have directed special efforts at disorder problems in the central districts of their cities (Skolnick and Bayley, 1986; Reiss, 1985). A number of studies have evaluated programs attempting to reduce crime and fear, in part, by reducing disorder. These evaluations provide insight into the benefits of targeting disorder.

The Police Foundation's fear reduction projects (Pate et al., 1986; Skogan and Wycoff, 1986; Skogan, 1990) carried out during the early 1980s in Houston and Newark utilized a number of strategies which showed promise in reducing disorder, fear, and, in some cases, crime. In Houston, strategies to

¹³To illustrate this trend, Skogan (1990: 89) reports that arrests for drunkenness, disorderly conduct, vagrancy, and suspicion numbered 2.3 million and constituted 52% of non-traffic arrests in 1960. By 1985, arrests for these offenses were down to 1.4 million and made up only 16% of non-traffic arrests. As Skogan observes, this decline seems even more dramatic in light of the population growth that occurred during this period.

increase police contact and cooperation with citizens and address area problems showed different degrees of success in reducing perceptions of physical and social disorder and fear. There is also evidence one of the strategies reduced property crime victimization (Pate et al., 1986: 29).¹⁴ Officers participating in each of these strategies reported addressing problems related to disorder.

The two fear reduction projects in Newark both involved disorder crackdowns, and both showed evidence of reducing perceived disorder and crime. One project combined community-oriented tactics like those implemented in Houston with a crackdown on street disorder (the crackdown measures involved foot patrol, radar and bus checks, roadblocks, and street sweeps). This produced significant reductions in perceived social and physical disorder as well as fear (Skogan, 1990: 117). Furthermore, the project significantly reduced total Part I crimes, personal crimes, auto theft, and outdoor crimes (Pate et al., 1986: 33). The other project combined the same disorder crackdown tactics described above (but no community policing tactics) with a physical clean-up effort. This produced a significant decrease in perceived social disorder and an insignificant reduction in fear (Skogan, 1990: 117). The program also led to significant reductions in total Part I crimes, personal crimes, and burglary (Pate et al., 1986: 32).

¹⁴Official crime data could not be analyzed because the Department had made recent changes in its reporting practices.

Another police effort targeting disorder was launched in the central business district of Oakland in 1984 (Reiss, 1985). This program utilized six strategies including foot patrol, increased enforcement of laws and ordinances against soft crime, and working with community agencies on problems relating to special populations (e.g., homeless and mentally ill persons) who contribute to disorder problems. The evaluation, though not intended to be rigorous, suggested the program reduced crimes against persons and property.

Similarly, the previously discussed foot patrol programs in Newark and Flint were intended to reduce disorder problems. Based on available evidence, it is difficult to say what officers did to reduce disorder and how much of it they did. Nonetheless, the Flint study showed crime reductions (the problems with these claims were alluded to earlier), and both studies indicated citizens felt safer and felt crime had been reduced as a result of the foot patrols.¹⁵ Esbensen (1987) evaluated a smaller scale foot patrol program implemented primarily to deal with disorderly behavior in a business district of a medium-sized southeastern city. Public disorder offenses were reduced (there was some evidence of displacement), but Index crimes were unaffected.

A six month crackdown on disorder and illegal parking in the Georgetown

¹⁵The Boston foot patrol study (Bowers and Hirsch, 1987) does not discuss officers' activities, and citizens were not surveyed for the evaluation. At any rate, the foot patrols produced no consistent changes in crime or community disturbances.

section of Washington, D.C. during 1985 did not produce measureable crime decreases, but opportunity samples interviewed in bars and at a major intersection revealed high percentages of those interviewed felt the area was safer, and a large percentage of those interviewed in the bars felt crime had been reduced (Sherman, 1990). Finally, Sampson and Cohen's (1988) analysis of robbery rates in 156 American cities found aggressive policing of disorder, as measured by arrests for disorderly conduct and driving under the influence, had a negative effect on robbery rates. Explaining this, they observe that "...the mechanism hypothesized to account for the results is the impact of police activities in changing the perceptions of potential offenders by controlling incivilities and disorder" (1988: 185).

In general, these studies suggest reducing disorder can have beneficial effects on crime and fear. At the same time, policing disorder raises a number of difficult dilemmas. First, defining disorder is a somewhat ambiguous task. Standards may vary from neighborhood to neighborhood, and the context within which the behavior happens must also be considered. Moreover, there are concerns about forcing the values of certain groups upon other groups not sharing those values. On the other hand, there is evidence urban residents tend to be in agreement over appropriate standards of behavior. Skogan (1990: 54-57) discovered personal characteristics such as race, class, and lifestyle explained very little variance in survey respondents'

ratings of disorder problems. In addition, there was substantial agreement across social groups (owners v. renters, white v. black, and young v. old) within neighborhoods. By the same token, Kelling (1987: 95) writes:

...it is deceptive to think that the majority of urban residents who are intolerant of street barbarism and support protecting neighborhood and traditional values are white, conservative, and middle class. Those demanding increased order are people of all races wanting to walk streets or ride buses without feeling under constant siege by others asserting their 'rights' to say anything or behave any way they wish.

Second, policing disorder creates more opportunities for discriminatory treatment, unequal enforcement, and extralegal conduct. Disorder enforcement also carries the potential for conflict with personal liberties and privacy rights. For example, Skolnick and Bayley (1986: 198-199) question the legality of sweeps used in Newark to disperse loitering groups who were not acting unlawfully. Further, aggressive crackdowns can spark resentment against police. They may also increase fear among citizens by appearing as signs that an area is dangerous. This happened in Newark where survey respondents who reported awareness of disorder crackdown tactics tended to be more fearful and more troubled by social and physical disorder in their neighborhoods (Skogan, 1990: 119).

Finally, many disorderly behaviors are not illegal. For instance, there is usually little police can do about drunken or mentally ill street people. And, as touched on earlier, there are few organizational incentives for police to

make arrests for minor offenses or to take street people to shelters or detoxification units (Skogan, 1990).

Despite these difficulties, it appears police will need to assume an increasingly important role in reducing disorder. Many scholars have commented on the breakdown of informal social control in modern society and the increasing reliance on formal mechanisms of social control. Empirical and anecdotal findings show that the communities most unable to successfully organize themselves and improve informal social control are usually the ones which have the worst crime and disorder problems (Sherman, 1985; Weisburd and McElroy, 1988). Additionally, informal social control may be ineffective with outsiders to an area and disorderly people in non-residential areas (Skogan, 1990: 169).

III. HOTSPOTS AND THE MINNEAPOLIS PREVENTIVE PATROL STUDY

BACKGROUND

Another line of research having implications for both patrol effectiveness and disorder reduction concerns the geographic distribution of crime. Routine activities theory (Cohen and Felson, 1979) states that crime does not occur randomly in time and space but, rather, is produced by the convergence in time and space of motivated offenders, suitable targets, and the absence of capable guardians.¹⁶ Sherman, Gartin, and Buerger (1989) sought to test this theory by studying the geographic distribution of crime in Minneapolis. They examined calls for service for a one year period and discovered 3.3% of the city's addresses and intersections generated 50.4% of all calls for which police cars were dispatched (1989: 37). Sherman et al. label these locations "hot spots." The concentration of calls among these hot spots was significantly greater than that which would be expected by chance. In addition, they examined calls for three predatory offenses which occur in public places (robbery, criminal sexual assault, and auto theft) and found these calls were even more concentrated than total calls. The predatory crime hot spots

¹⁶Routine activities theory was originally applied to only exploitative offenses like robbery and auto theft, but Felson (1987: 912) has since expanded the theory to cover mutualistic offenses (such as prostitution), competitive offenses (such as fights), and individualistic offenses (such as individual drug use).

produced high numbers of both predatory crime calls and total calls.

An important finding from an operational standpoint is that the hotspots tended to be bunched near one another. The data revealed "...420 clusters of addresses of three or more hard crime calls, totalling 20 such calls, located within one-half block of each other" (1989: 43). Further, 72 of these clusters were within two blocks of each other.

The discovery that 3% of a city's addresses and intersections produce 50% of the city's calls for service has been replicated in Kansas City (Sherman, 1992) and Boston (Pierce, Hyatt, Spaar, and Briggs, 1988). Along the same lines, Pierce and his colleagues (1988) discovered high levels of short-term recurrence for gang-related problems and, to a lesser extent, car accidents at certain blockfaces and intersections within Boston. Evidence from other types of studies also reveals concentration of crime in specific places. Studying crime and fear in four Chicago neighborhoods, for example, Lewis and Maxfield (1980) found that certain streets, blocks, and intersections were responsible for disproportionate amounts of crime in the neighborhoods and that residents accurately perceived these locations as dangerous places.

These findings suggest hot spots are good foci for crime control efforts. If disorder contributes to crime, curbing disorder at these crime-prone locations may be a means to reducing the amount of crime they produce. Indeed, hot spots are often areas where social disorder can be particularly troublesome.

The number one predatory crime hot spot in Minneapolis was an intersection with bars, a liquor store, and a park (Sherman et al., 1989: 45). The second ranking predatory hot spot was a bus depot. These types of locations attract many people and present opportunities for rowdy and otherwise troublesome behavior. They also have high levels of anonymity. Surveys show people are troubled by disorder in public areas like parks and shopping centers, yet informal methods of social control are arguably not as effective in such places (Skogan, 1990: 47,169). Therefore, reducing disorder at public area hotspots through formal mechanisms (i.e., police) should be less controversial and more favorably received, especially by business people and shoppers.

Considering that these locations appear to possess social and physical features which facilitate disorder, displacement of disorder to other places may not occur to a great extent. Even if disorder is displaced, it may be displaced to areas not as conducive to more serious crime (Sherman and Weisburd, 1988).

THE MINNEAPOLIS EXPERIMENT: SELECTION AND TREATMENT

In response to the hot spots findings in Minneapolis, Sherman and Weisburd (1988; 1990), in conjunction with the Minneapolis Police Department, designed and implemented the Minneapolis Preventive Patrol Experiment. The experiment ran from December 1, 1988 to November 30,

1989 and examined the effects of preventive patrol at 110 hot spots. A detailed description of the research design and procedures is provided in Sherman and Weisburd (1988). For operational purposes, a hot spot was defined as a cluster of addresses which together produced 20 or more hard crime calls (e.g., robbery, rape, burglary) and 20 or more soft crime calls (e.g., disturbances, prostitution) over a one year period.¹⁷ The selection year was from June 1987 to June 1988. The hot spots had to be locations where crime occurred in public (public places or places with spillover of activity into streets or parking lots) so that police presence could reasonably be expected to have a deterrent effect. The researchers chose to exclude the following types of places:

...all residential and most commercial buildings over 4 stories (including two hotels), almost all parking garages, department stores and indoor malls, public schools, office buildings, residential social service institutions (such as homeless shelters), hospitals, police stations and fire stations. Parks were also excluded because they have their own police. Finally, a few known 'magnet phone' locations, at which events occurring elsewhere are routinely reported, were excluded (Sherman and Weisburd, 1988: 17).

The boundaries of the hot spots were defined so that the entire location could be viewed from an epicenter. Independent field workers visited each site and configured the boundaries according to the following general

¹⁷Hot spots were identified using 911 calls. See Sherman et al. (1989) for a discussion of the benefits and limitations of using call data as crime measures.

principles: the hot spots could not be longer than one standard linear street block; the hot spots could not extend more than half a block from either side of an intersection; and no hot spot could be within one standard linear block of another hot spot.

Finally, the selected hot spots had to show some stability in the number of hard and soft crime calls they generated over a two year period. The researchers examined calls from June 1986 to June 1987 and June 1987 to June 1988. "Clusters with over 150% increases or 75% decreases in hard crime calls from one year to the next were excluded from the possible sample..." (Sherman and Weisburd, 1988: 18).

Rankings among the eligible hot spots were based primarily on hard crime calls. For practical reasons, only the top 110 locations were used for the study. The high crime addresses within these hot spots produced 20,118 crime calls (6,260 hard crime calls and 13,858 soft crime calls) during the selection year, averaging 182.9 per cluster with a minimum of 56 and a maximum of 628. Counting all addresses within the clusters (including those which were not high crime addresses), the clusters contained a total of 1,663 addresses with an average of 15 addresses per hot spot. These locations produced 39,015 total calls (crime and noncrime) during the year before the experiment began (December 1987 through November 1988) for an average of 355 calls per hotspot. This constituted 10.8% of all calls dispatched in Minneapolis

during that time period.

The hot spots were randomly assigned to the treatment or control group, and those assigned to the former group received greater dosages of patrol over the course of the experiment. Between the hours of 11 a.m. and 3 a.m., officers assigned to the experimental locations spent their uncommitted patrol time at these locations, thereby providing "intensified but intermittent patrol" (Sherman and Weisburd, 1988: 25). Activities conducted at the hot spots, if any, were left to the discretion of the officers and their supervisors. Police presence at the experimental locations was increased about 13% during the experiment (these hot spots received about 3 times as much patrol presence as the control hot spots), resulting in a modest decrease in crime calls at these locations (Sherman and Weisburd, 1990).¹⁸

THE DATA: OBSERVATIONS

The data used in this study are observational data collected during the experiment.¹⁹ Throughout the course of the project, trained observers were

¹⁸The most reliable data from the experiment extend from the beginning of the project to the summer of 1989. These data indicate a relative decrease of about 13% in total crime calls at the experimental hot spots. Published results from the experiment will be forthcoming from Sherman and Weisburd.

¹⁹The data were provided to the author in their original form by the Crime Control Institute, Washington, D.C. Crime Control Institute staff assisted the author in constructing new data sets which were used in the

sent to the 100 most active hotspots on a random assignment basis to conduct 70 minute observations between the hours of 6:30 p.m. and 2:30 a.m. (coinciding largely with the 7:00 p.m. to 3:00 a.m. window which generates the most calls for service). This was done to verify officers' reports of the time they spent at the hot spots and to examine differences in patrol dosage and street disorder at experimental and control hot spots. The observations were performed during 13 periods, each of which lasted 28 days. This way, observations at each location were spread evenly across the year. The observations took place Tuesday through Saturday nights, the most active nights of the week. They were conducted "...in systematic rotating order for reasons of administrative convenience, with three nights each week observed: Tuesday-Thursday, Wednesday-Friday, Thursday-Saturday, and so on in sequence" (Sherman and Weisburd, 1988: 32).

Twenty-nine observers participated in the data collection.²⁰ Project staff recruited these people through newspaper ads and gave them three days of training. To enhance the validity and reliability of the observations, training exercises required participants to code material from videotapes and written scenarios. Further, supervisors made unannounced field visits to verify that

analysis (the newly constructed data sets are described in the analysis section).

²⁰Observers worked both full-time and part-time. There was very little turnover, but the staff was expanded during the course of the project (the observers did not all participate from the beginning of the project).

observers were conducting observations when assigned and to double-code presences and disorders.²¹

This investigation uses data from 6,273 observations conducted between December 1, 1988 and November 30, 1989.²² Thus, the sample has about 63 observations per hot spot. Including the first and last minutes of each observation, the data contain an average of 71 minutes per observation. This constitutes an average of 74 observation hours at each hot spot for the whole year.

The observers recorded police presences and disorderly behaviors which they witnessed at the hot spots. (Key descriptive statistics regarding observations, police presences, and disorders are presented in Appendix A. Descriptive statistics pertaining to police presences are taken from the police "block" file described in the second section of the analysis chapter.) Observers recorded both events within the hot spots and events outside the

²¹However, systematic tests (i.e., reliability scales) were not conducted on the reliability of the observation instruments.

²²The observers conducted a total of 6,465 observations. However, 192 (3%) of these observations are excluded from the study due to missing information or conflicting data regarding the beginning and ending times of police presences, disorders, and/or the observations. These observations contained 5% of the total observed disorders and 4% of the observed police presences (before combining overlapping presences).

Examination of these observations across months revealed the proportions of excluded observations in December 1988 and January 1989 (the first two months of the project) were slightly higher than those of the other months. Examination of these cases by time of day and experimental/control group status revealed no marked differences.

boundaries of the hot spots but within visual distance. When recording police presences, the observers counted separate presences as opposed to counting numbers of officers. One police squad car represented one police presence regardless of how many officers were in the squad. Similarly, observers would code a pair of officers walking together as one presence. In addition to recording arrival and departure times of police, the observers recorded whether the presences were stops or drive-bys.²³ Off-duty police working as security guards were recorded as police presences if they were working in uniform. The observation sample contains information on 24,813 police presences which were observed over the course of the year. The vast majority of these presences (21,733) were drive-bys. The average length for all police presences was 3 minutes, while the average length for stops was 14 minutes.²⁴

The observers also recorded several different types of criminal and otherwise disorderly behaviors: solicitation, drug transactions, physical

²³If the officers stopped, the observers recorded any activities the officers conducted using categorizations such as traffic law enforcement, foot patrol, entering building, verbal citizen contact, and arrest (see Appendix B). Observers also recorded the presence (but not activities) of security guards, fire department vehicles, and ambulances.

²⁴Because police presences were not measured in seconds, the length of each presence is counted as the number of minutes in which the presence occurs. Thus, a drive-thru from 8:00 to 8:00 is counted as 1 minute with police presence even though the presence lasted for less than 60 seconds. Lengths of disorders are calculated in the same fashion.

assaults, auto or building break-ins, vandalism, verbal disorders (e.g., loud shouting or verbal harassment of passersby), loud disputes, drunk or drugged behavior, loud noise or music, the presence of bag persons, and persons down (as if drunk, ill, or injured). An "other" category was created to record other types of disorderly conduct (such as urinating in public or flashing) or more serious crimes (such as robbery or rape) which were not included on the observation sheets. (Descriptions of these behaviors and instructions for coding them are included in the codebook in Appendix B.)²⁵

The data capture 4,014 observed disorders. These disorders include 418 criminal events (solicitation, drug transactions, physical assaults, auto or building break-ins, and vandalism) and 3220 noncriminal events.²⁶ The remaining 376 disorders were either unidentified or classified as "other." Most of these disorders were relatively brief events, lasting an average of 4 mins.

Defining disorder involves a certain degree of subjectivity on the part of researchers. Coding particular instances of disorder compounds this problem

²⁵Observers were also supposed to record whether a disorder involved one person or more than one person; however, they did not record this in many instances.

²⁶If a criminal act and a noncriminal act appear together as part of the same disorder (for instance, an act of vandalism accompanied by a verbal disorder), the disorder is counted as a criminal disorder only. Noncriminal disorders accompanied by unidentified disorders are not classified as criminal or noncriminal since the unidentified behaviors could have been criminal.

by introducing an unknown amount of observer bias. In addition, there is no available information indicating how users of the hot spots perceived these behaviors. Nonetheless, the behaviors recorded by the observers are consistent with those cited as problematic behaviors in the disorder literature. Skogan (1990), for example, discusses the concern which survey respondents in 40 city neighborhoods expressed over problems like public drinking, street harassment (verbal and physical), drug use and sales, vandalism, and noisy neighbors. He also comments on disturbances created by street people, a substantial number of whom may be mentally ill and exhibit bizarre behavior. Moreover, his discovery that people tend to agree over definitions of disorder lends credibility to efforts to define, observe, and record such behavior. Finally, the training and reliability checks discussed previously were done to increase consistency and minimize bias in the identification of disorderly behaviors.

These data hold much promise for deterrence research. They capture many disorderly events not included in official data and do not suffer from reporting effects and potential tampering problems as do official data. Furthermore, the data present observed levels of disorder rather than perceptions of disorder levels. Police visibility, too, is measured directly rather than inferred from other information. More importantly, the data allow for the study of threat communication and the reactions of criminal and

disorderly persons to environmental cues (i.e., visible police presence), a line of deterrence research advocated by Cook (1980: 260). In a sense, they also provide a target-specific criminal opportunity study (Cook, 1980: 243). Police presence serves as an attribute characterizing criminal and disorder opportunities at the hot spots.

IV. POLICE PRESENCE, DISORDER, AND DETERRENCE: HYPOTHESES AND CONCEPTUAL FRAMEWORK

This study examines the impact of police presences upon disorder both during and after the presences. Before examining specific hypotheses, it is important to note that the independent variable in this study is police presence and not any particular style of policing or set of police activities (in contrast to the disorder reduction efforts discussed previously). This is true for the experimental hot spots as well as for the control hot spots. Even without special measures, there is reason to believe police presence has a deterrent effect on social disorder. Reiss (1985: 29-30) discusses the benefits of increasing patrol presence in disorder-prone areas:

Perhaps one of the ways that the police can more effectively control soft-crime is by increasing their presence in situations where they wish to control the incidence of soft-crime or its consequences. By being present, they either increase the risks of potential offenders to the point that offending is thwarted or their presence increases the probability that those being observed can be arrested for some infraction of the law.

This should be especially true at hot spots. Whether in a squad or on foot, officers project greater visibility in the small geographic area of a hot spot. Likewise, officers can survey these locations more easily. These factors create a more potent police presence.

However, the presence of an officer may not deter social disorder if it does not signal a change in the objective probability of apprehension (Zimring

and Hawkins, 1973: 164). In other words, disorderly persons may not feel threatened if police tend to ignore many disorderly behaviors. If this is so, the presence of police will not decrease disorder unless officers place an emphasis on disorder enforcement.

This issue is especially salient to noncriminal disorderly behaviors and behaviors prohibited only by seldom-enforced municipal ordinances. Nonetheless, when such conduct occurs in the presence of officers it can provoke unwanted encounters with police. Officers may choose to enforce applicable ordinances or simply question disorderly persons. Gibbs (1975: 106-107) argues that some people perceive interrogation by police as a form of punishment. Similarly, a Police Foundation study in San Diego (Boydston, 1975) claimed that field interrogations decrease certain types of crimes. Because over 98% of the field interrogations in the study did not result in arrests, the author attributed the crime decreases to the deterrent effect of the field interrogation process. Therefore, there is reason to believe such encounters with police have punitive value. In the context of this research, visual proximity to police may raise subjective assessments of the probability of enforcement or field interrogations, thus communicating some level of threat and deterring disorderly conduct.

There are still other reasons why the presence of police might not deter some forms of disorderly behavior. Deterrence scholars have commented on

the difficulty of deterring violations which involve conflicts over proper standards of behavior as well as those that occur in situations where people are highly emotional or under the influence of drugs or alcohol (Andenaes, 1974; Zimring and Hawkins, 1973). This certainly applies to many forms of social disorder. Also, disorder is more common in poor areas (Skogan, 1990: 51). Conventional wisdom holds that members of the lower class are less susceptible to deterrence because they have less to lose from legal punishment (in terms of both formal and informal costs), and they tend to be less concerned about future consequences of their behavior (Geerken and Gove, 1975: 509; Zimring and Hawkins, 1973: 127-128).

Nevertheless, Cook's (1980) notion of limited rationality argues that people adopt rule-of-thumb principles which guide their decision-making even when they are very emotional or inebriated. If this is correct, such principles should moderate the behavior of many people, especially when police are around and there is more opportunity to get into legal trouble. And, as Zimring and Hawkins (1973: 128) observe, legal intervention can make life worse for people of all classes. Gibbs (1975: 211-212) points out that arguments regarding the non-deterrability of certain types of people and offenses are based on intuitive notions, but have little evidential support. More recently, Sherman and Smith (In press) have demonstrated that deterrability varies among people in poor neighborhoods, at least in regards

to sanctions for domestic violence.

At any rate, it is obvious deterrence operates at a certain level even under unfavorable conditions. The question here is whether immediate and/or recent visual cues of enforcement (police presences) raise perceptions of the certainty of legal intervention and produce marginal increases in deterrence of criminal and disorderly acts at troublesome locations where the potential benefits of greater deterrence are substantial.

This study investigates the issue using Sherman's (1990) distinction between initial and residual deterrence associated with police crackdowns. Sherman (1990: 7) identifies a crackdown as an increase in the certainty or severity of official police reactions to specific types of offenses or all offenses in a specific area. Initial deterrence is the decrease in offending that occurs while a crackdown is in effect. Residual deterrence refers to effects which continue after a crackdown has ended. According to Sherman, crackdowns create residual deterrence by raising uncertainty about risk. This causes potential offenders to overestimate risk levels. Even after a crackdown has ended, heightened risk perceptions may take time to decay. Consequently, the withdrawal of a crackdown should not lead to an immediate return to pre-crackdown offending levels. Sherman finds support for this idea in his review of police crackdowns. Further support comes from earlier analyses performed with the data used in this study (Sherman and Weisburd, 1990). The increase

of intermittent, unpredictable police presence in the experimental areas, which approximated a crackdown-backoff pattern (Sherman, 1990: 39), resulted in 50% less observed disorder at the experimental hot spots relative to the control hot spots. Ongoing analyses by Sherman and Weisburd show that the proportion of minutes with disorder in the experimental areas was 100% less than that in the control areas.

This application treats each observed police presence at a hot spot as analogous to an area crackdown. The analysis of initial deterrence, or what might be more appropriately called direct deterrence in this context, tests if there was less disorder at the hot spots during minutes when police were present. This portion of the analysis also makes a general assessment of residual deterrence by testing whether the increase in patrol levels at the experimental locations produced residual effects which reduced disorder during times when police were not present at the hot spots. This concept has not been explicitly tested by other patrol studies. Next, the analysis examines specific instances of police presence and determines if stronger dosages of police presence (as measured by the length of the presences) create residual deterrent effects. Testing this issue involves analysis of the time from when police leave a hotspot until the next disorder occurs.

If specific instances of police presence do create residual effects, one would expect longer police presences to produce longer follow-up periods

without disorder. Longer presences may increase perceptions of police concern about the area and raise uncertainty regarding police intentions and actions. That is, the longer police remain in the area, the more likely it is that people around the hot spot will think the police are "up to something" or are on the watch for criminal or disruptive behavior. This should increase perceptions of risk. This alone may drive potential trouble-makers from the area, creating a more lasting residual effect. Of course, police may also move trouble-makers out of the area by direct contact. Friends and associates of these people might also learn by word of mouth that the area is or has been under police guardianship. If potentially troublesome persons are not driven from the hot spot, longer presences may still make them more cautious and less disorderly for some length of time. A parallel exists in psychological research showing that longer exposure to films of fatal car accidents strengthens favorable attitudes toward safe driving practices, though this effect disappears over time (Leventhal and Niles, 1965).

On the other hand, longer police presences may not drive criminal/disorderly persons from the area or make them more cautious. Disorder may simply begin or resume as soon as police leave (assuming deterrence operates while police are there). Further, the occurrence of disorder could be a function of the flow of people through the area. Soon after police leave a hot spot, the next potential trouble-maker(s) may enter

the area without any knowledge of recent police presences.

Yet assuming longer police presences do lengthen residual deterrence, is there a point at which longer police presences provide no additional benefits? Sherman (1990) theorizes that there is a maximum utility point for crackdown length, beyond which there are no additional gains in residual deterrence. Such a finding here would imply there is an optimum presence length for raising risk perceptions and driving trouble-makers away from a hot spot. Once a presence has passed that length, there is little additional benefit from staying longer.²⁷ There is as yet, however, no empirical basis for estimating where that threshold may lie, if it exists.

²⁷This discussion does not explicitly consider the benefits of actions police might take during long stops (e.g., talking with citizens about problems, conducting foot patrols, or confronting suspicious and troublesome persons). Since the hot spots experiment did not implement specific styles of police activity, this should not pose a major problem. The longer police are at a location, however, the more likely it is they will engage in these types of activities (regardless of whether the stop is proactive or reactive). In addition to any direct results, the perceptual effects of such actions may heighten uncertainty and enhance deterrence. On the other hand, troublesome persons might not feel threatened if officers appear preoccupied with other matters (as when officers are responding to a call). The data do not permit clear inferences in this regard. This study treats police activities and their potential benefits as properties inherent to longer police stops.

V. ANALYSIS

DIRECT DETERRENCE AND GENERAL RESIDUAL DETERRENCE

Assessment of direct deterrence involves comparing the amount of disorder which occurred during minutes having police presence with the amount which occurred during minutes with no police presence. If police presence directly deters disorder, minutes with police presence should have significantly (and substantially) less disorder than non-police minutes. However, this task is complicated by a number of factors. There are undoubtedly cases in which police presence at a hot spot was in response to an observed disorder or something else which was contributing to disorder.²⁸ This increases the proportion of police minutes overlapping with disorder and creates a cause and effect problem. Unfortunately, the data do not allow identification of these instances. To lessen the problem, the analysis compares the proportions of police and non-police minutes having one or more disorder initiations.²⁹ This allows examination of the extent to which

²⁸In the police presence file described in the next section, 7.9% of the police stops overlapped with one or more minutes containing an observed disorder. The corresponding figure for drive-by presences was only 3.8%.

²⁹As an additional check on the cause and effect issue, correlations were run between the duration of each presence (using the police presence file discussed in the next section) and the rate of disorder initiations per observed minute for the hot spot where each presence occurred. The correlations were

police presence prevents the outbreak of new disorders. Of course, this does not resolve all ambiguities. If an observed disorder began while police were at a hot spot, it is still possible the police presence was in response to a problem which was initially hidden from the observer's view.³⁰

Notwithstanding, these measures are less susceptible to cause and effect problems than are comparisons of the proportions of police and non-police minutes during which any disorder is occurring.

The analysis uses a minute-based file which contains a record for each observation minute at every hot spot (N=432,719). The file does not include the first and last minutes of each observation because it is not clear these minutes were always observed in their entirety (nuances in the coding schemes also made this necessary).³¹ Each record contains variables

run only for presences observed in their entirety, and they were examined with and without drive-by presences. These correlations were negligible; the strongest was -0.16. Also, correlations between the drive-by indicator variable and the disorder initiation rate were very small. This reduces the magnitude of the cause and effect problem by showing high disorder hot spots were not attracting longer police presences in the overall sample.

³⁰For example, police might have responded to a barfight and dragged the fighting parties outside. If the parties were still behaving in a disorderly fashion (e.g., yelling or fighting with the officers) when the officers brought them out, the incident would appear as a disorder which began during police presence.

³¹If a police presence or a disorder was in progress at the beginning of an observation, the observer used the start time of the observation as the beginning time of the presence or disorder. In these cases, it is unclear when the presence or disorder really began. Also, observers recorded the ending time of an observation as the ending time of any police presence which was

indicating how many separate police presences occurred during the minute, how many of those presences began in the minute, and how many ended in the minute. The same information is available for disorders. Altogether, 53,097 (12%) of the minutes had at least one police presence occurring during part or all of the minute and 13,921 (3%) had at least one disorder occurring during part or all of the minute. In the figures presented below, however, a police minute is defined as a minute which had police presence throughout the entire minute. This is necessary because, within a given minute, it is impossible to determine if the beginning of a disorder occurs in the presence of police unless it can be shown police were there for the entire minute. This restriction excludes those minutes in which all police presence(s) began and/or ended and leaves a sample of 30,372 police minutes.³² A non-police minute, in contrast, is a minute which had no police presence at all.

Table 1 compares the proportion of police minutes having disorder

ongoing at the end of an observation. This means that for the first and last minutes of an observation, one cannot determine whether police were present for the entire minute. These coding rules have implications for the analyses below.

³²This stipulation has a substantial effect on the results of these comparisons. To illustrate, the proportion of police minutes having disorder initiations is 0.00895 when all minutes having any police presence are included. This is slightly higher than the proportion of non-police minutes having disorder initiations. Nevertheless, it is impossible to ascertain whether the disorders in these minutes began before, during, or after the police presences.

initiations to the proportion of non-police minutes having disorder initiations. (Throughout the analysis, disorder refers to total disorders and includes both criminal and non-criminal acts. Also, every new disorder counts as a disorder initiation regardless of whether there was already another disorder in progress during the minute.) Both of these proportions are very small, constituting less than 1% of their respective groups. The figures show the proportion of police minutes with disorder initiations is 21% less than the proportion of non-police minutes with disorder initiations, a statistically significant difference.³³

Next, the analysis focuses on only those disorders which were clearly criminal acts: solicitation, physical assaults, drug activity, building or auto break-ins, and vandalism. These acts are arguably more deterrable than the other recorded disorders. They are criminal acts which clearly fall under police jurisdiction; certainty of punishment (or at least certainty of police intervention) should be greater for these acts than for the other disorderly

³³For the sake of argument, the proportions of police and non-police minutes having any disorder were calculated and compared (despite the cause and effect problem discussed previously). The first run counted any minute with any type of police presence (drive-by or stop) as a police minute and any minute with any disorder as a disorder minute. The proportion of police minutes with disorder was 0.03164 while the corresponding figure for non-police minutes was 0.03225. Thus, police minutes only had 2% less disorder than non-police minutes. However, about 19% of the police/disorder overlap minutes involved circumstances which make it impossible to determine whether or not the events truly overlapped (e.g., a drive-by and a disorder occurring in the same minute). Therefore, another run was conducted using only those minutes in which police were present for the entire minute. The proportion of these minutes having disorder was .02812, a figure 13% less than that of non-police minutes ($p < .001$).

behaviors. Also, criminal acts such as drug activity and solicitation are less likely to result from emotional outbursts and are more likely to be conducted in a rational and calculated manner, making them theoretically more amenable to deterrence.

Table 2 provides the figures for crime initiations. The proportions of police and non-police minutes with crime initiations are both only a small fraction of a percent, but the proportion of police minutes with crime initiations is 32% less than that of non-police minutes. This represents a statistically significant difference.

Although police presence directly deters crime initiations, the effect is not as large as we might expect. Inspection of these cases revealed the 19 crimes beginning during police presence primarily involved solicitation, drug activity, and physical assaults (there was also one building break-in). The physical assaults which began during police presence may have been the result of emotional outbursts which made them more difficult to prevent (there were no indications these physical assaults were accompanied by other criminal acts such as robbery or sexual assault). In fact, some may have been in response to police intervention. The instances of solicitation might also be understandable if police tend to be lax on enforcement of this offense. Another explanation is that the officers were not immediately visible to the offenders. However, an important caveat must be noted. In many instances

when police were not present, potential offenders could have noticed the presence of observers and viewed them suspiciously. This may have deterred criminal activity during times when police were not present.

Despite this problem, police presence creates a direct and statistically significant reduction in crime. Moreover, the magnitude of this effect is larger than that for overall disorder. This indicates deterrence (or displacement) is in fact stronger for the acts that are theoretically the most deterrable (and the most serious).

Another interesting question concerns the direct effects of police presence in the experimental hot spots as compared to the control hot spots. This can be illustrated by comparing disorder initiations during police minutes in the experimental and control areas to disorder initiations during minutes without police in the control areas. In essence, the level of disorder during non-police minutes in the control areas represents a baseline level of disorder for times when police are not present in areas which have normal patrol levels. Put another way, this represents the usual level of disorder at hot spots. Referring again to Table 1, police minutes in the control areas had disorder initiations 6% less often relative to non-police minutes in the control areas, and this difference is not statistically significant. In contrast, the proportion of police minutes having disorder initiations in the experimental areas is 34% less than that for the baseline minutes. This difference is almost six times

greater than the difference between police and non-police control minutes, and it is highly significant.

Similar results can be obtained for crime initiations (see Table 2). Police minutes in the control areas produced a 17% direct reduction in crime initiations relative to non-police minutes in the control areas. This difference is not statistically significant, but this may be due to a lack of statistical power caused by the low rate of crime initiations and the reduction in sample size which occurs when examining only control area police minutes. Nevertheless, comparison of crime initiations during police minutes in the experimental areas and non-police minutes in the control areas reveals a pattern much like that found for overall disorder. The proportion of police minutes in the experimental areas having crime initiations is 55% less than that of the baseline minutes, and this difference does achieve statistical significance. This direct effect is over three times higher than the direct deterrent effect exhibited by the control area police minutes.

These analyses of direct deterrence across experimental and control areas indicate that direct deterrence was achieved in a much more substantial and significant way by police presences in the experimental areas. Further, the direct deterrent effects of police presence found in the overall sample appear to be due primarily to the direct effects of police presence in the experimental areas. This may be attributable to the higher proportion of proactive

presences in the experimental locations. In other words, police presences in the control areas were more likely to be in response to problems. Under these conditions, it seems more likely that police could bring a previously unobservable disorder into the observer's view or that some other problem, aggravated by police intervention, could create a chain reaction leading to an observed disorder.

A second interpretation is that the experimental presences had a reinforcing effect on one another. Such reinforcement could have been caused by the greater frequency and duration of police presences at the experimental locations. This implies that increasing patrol levels around a hot spot enhances the direct deterrent effect of police presence through some kind of a cumulative process whereby users of hot spots become more sensitive to and aware of police presence. This improves deterrence during specific instances when police are at the hot spots. Comparing disorder and crime initiations during police and non-police minutes in the control areas shows the direct deterrent effects produced by police presence when an area has normal patrol levels. Comparing the baseline level minutes (i.e., minutes with no police in the control areas) with experimental area police minutes, in contrast, demonstrates the amount by which police presence directly reduces usual disorder levels when the overall level of preventive patrol in the area is higher.

An overall assessment of residual deterrence can be made by contrasting the amounts of disorder and crime during non-police minutes in the experimental and control areas. Relative to minutes with no police presence in the control areas, non-police minutes in the experimental areas had 18% less overall disorder initiations (see Table 1) and 40% less crime initiations (see Table 2). We again find that patrol has greater effects on crime than on overall disorder, and both of these difference are statistically significant.

A possible explanation for these residual effects is that a higher percentage of the non-police minutes in the experimental areas were in the wake of a police presence. In other words, each instance of police presence may be followed by a certain number of minutes during which there is residual deterrence created by the presence. Because the experimental areas had more police presences, a higher percentage of their non-police minutes were minutes in which residual deterrence may have been operating from a recent instance of presence.

Residual deterrence may have also built up through a cumulative process like that hypothesized earlier. Indeed, a cumulative effect created by the increased frequency and duration of police presences at the experimental locations could have improved deterrence both during minutes with police and minutes without police at these locations. For instance, residual deterrence from one or more previous presences might still have been

operating at the time of a new presence. The new presence would benefit from the residual effect of the previous presences (direct deterrence produced by the new presence would appear stronger), and the new presence might add some reinforcement to the residual effects of the preceding presences. Thus, the direct and residual effects of specific instances of presence may have been reinforced by previous presences and, at the same time, enhanced the effects of later presences. Under this explanation, much of the improvement in direct deterrence at the experimental locations could, in fact, be attributed to residual deterrence spilling over into police minutes.³⁴

The results clearly demonstrate that increasing the level of police presence in an area enhances deterrence in that area both when police are around and when they are not around. In these data, the residual deterrence produced by increased patrol levels (in terms of both overall disorder initiations and crime initiations) was actually greater than the amount of direct deterrence achieved with normal patrol levels. These analyses support, but do not directly test, the theory that patrol levels affect potential offenders' perceptions of risk in an area. Though the demonstration of residual deterrence essentially affirms common sense notions, it constitutes a very direct, experimentally-controlled demonstration (and perhaps the most powerful test to date) of residual deterrence created by police patrol.

³⁴This interpretation has been suggested by Lawrence Sherman in conversation with the author.

POLICE DOSAGE LEVELS AND RESIDUAL DETERRENCE

This section explores residual deterrence in a different fashion. Specifically, it utilizes continuous time, parametric event history models, alternately called survival models or failure time models, (Allison, 1984; Elandt-Johnson and Johnson, 1980; Kalbfleisch and Prentice, 1980; Lawless, 1982) to analyze time from police departures to the next observed disorders. As stated previously, the objective is to determine if longer police stops create residual deterrence in the form of longer survival times without disorder.

The investigation uses a police instance-based file indicating the beginning and ending times of each observed police presence and whether or not the presence was a stop or a drive-by (this file initially had 19,498 cases). More specifically, the observations correspond to blocks of police time. A "block" of police time refers to consecutive minutes with at least one police presence. Because the data do not facilitate sequential ordering of presences and disorders in time units smaller than one minute, it was necessary to combine overlapping and adjacent presences into blocks of police time. In some cases, therefore, an observation may represent more than one separate presence. If, for instance, a squad car was present at a hot spot from 8:00 to 8:10 and a second squad stopped at the same hot spot from 8:04 to 8:07, the second squad would not appear in the data as a unique presence. Rather, it would

appear as part of a non-drive-by presence from 8:00 to 8:10. Similarly, a squad car arriving at 8:00 and leaving at 8:05 followed by another police stop from 8:06 to 8:10 would constitute a block of non-drive-by presence from 8:00 to 8:10. The same applies to drive-by presences. A drive-by at 8:00 followed by a drive-by at 8:01 would appear as two minutes with drive-by presence.

The event history models presented below use a subsample of the total police blocks (hereafter referred to as presences). Presences were excluded if they were not observed in their entirety, or if they did not have at least one minute of follow-up time after they ended. This mandated the exclusion of cases in which police arrived or were already present when the observer arrived and cases in which police presences occurred or were still continuing when the observer left.³⁵ Since the research staff randomly determined the starting and ending times of the observations, this criterion acted as a random variable by which cases were removed, though (as one would expect) this affected stops to a much greater extent than drive-by presences. This restriction resulted in the removal of 1,298 cases (6.7% of the sample).

A number of other conditions also determined the sample for the analysis. To obtain a better measure of presence duration, presences involving stops

³⁵If police arrived at the same minute the observer arrived, the presence still had to be excluded because the data define presences as blocks of consecutive minutes with police presence. In these cases, there is no way of knowing whether there was any police presence in the minute before the observer arrived.

were dropped if, in addition to the stops, the presences were continued by one or minutes during which there were only drive-by presences.³⁶ The study also does not include presences whose follow-up periods had a disorder and a new presence occurring in the same minute since it is impossible to determine whether these follow-ups ended with new disorders or new presences. Hence, it is unclear whether such a case represents the end of residual deterrence for one presence or a lack of direct deterrence for the new presence. In addition, presences over 20 minutes in duration were excluded for reasons of statistical power which are elaborated below. Together, these requirements resulted in the exclusion of 509 cases (this represented 2.8% of the cases remaining at that point).

Finally, presences were removed from the forthcoming survival analysis if they had disorder occurring during any minute of the presence (including the first and last minutes of the presence). These cases have a high failure rate, and many fail quickly. Overall, 67% of the cases having disorder during any minute of the presence experience a disorder failure in the first minute after police leave. This suggests many of these presences did not have a direct deterrent effect. Disorder may have begun or continued despite police presence or had a contagion effect causing disorderly conditions to linger

³⁶For example, a stop from 7:00 to 7:02 followed by a drive-by at 7:03 and another stop from 7:04 to 7:10 would appear as a police presence from 7:00 to 7:10. Likewise, a stop from 7:30 to 7:35 followed by a drive-by at 7:36 would appear as a police presence from 7:30 to 7:36.

after the police presence. Either way, it appears cases overlapping with disorder have more bearing on direct deterrence and whether police presence restored order to a disorderly environment. (Though due to the measurement issues mentioned previously, it is not always clear whether or not these presences and disorders actually overlapped with one another.)

Police stops do better in this respect than do drive-bys. Of those presences remaining in the sample at this point, there were 68 stops which had some overlap with observed disorder. Thirty-four (50%) of these 68 cases had observed disorder failures, and 76% of these failures came in the minute after the presence. In contrast, 625 of the remaining drive-bys had some overlap with observed disorder and 492, or 79%, of these 625 cases had disorder failures. Further, 91% of these failures occurred in the minute after the presence.

Removing these presences decreased the sample by 694 cases (4% of the remaining sample at that point). Unfortunately, this also meant losing 526, or 30%, of the remaining failures. Nevertheless, it appears there was not an orderly environment at the time the police left in many of these cases. In other cases, the situation is ambiguous. It does not seem appropriate to test the residual deterrent effects of a police presence if that presence does not first demonstrate direct deterrence by maintaining order or restoring order to a disorderly environment. Removing these cases makes the tests of residual

deterrence less ambiguous and more informative by ensuring the environment was orderly at the moment police left.

This left a final sample of 16,997 presences (87% of the original sample), 16,050 of which are drive-by presences only. The remaining 947 are stops under 20 minutes. The average length of these stops was about 7 minutes. (See the descriptive statistics presented in Appendix A.) The follow-up period for each presence begins the minute after the presence ends. If a disorder was observed before the end of the observation and before the next observed police presence, the case has a disorder event (or, a disorder failure) at the time of the disorder. If there was not a disorder failure, the observation is censored at the time of the next police presence or the end of the observation, whichever comes first. When an observation is censored, the event history models take into account the fact that the case survived at least to the time of censoring.

The exclusion of presences greater than 20 minutes is related to this point. By definition, longer presences tend to have less potential follow-up time. If, for example, an observation ends at 9:10, a drive-by at 8:30 has 40 minutes of potential follow-up (barring any censoring due to another police presence), whereas a 25 minute presence beginning at 8:30 has only 15 minutes of potential follow-up. This means the drive-by has more at-risk time. On the other hand, the rarity of observed disorder (on average, less

than one disorder per 70 minute observation) may actually create a bias against finding superior residual deterrence for the longer presence. When there is censoring, the estimation procedures for the model can only take into account that the observation survived at least to time T without a disorder. Because the potential time T is systematically smaller for longer presences,³⁷ the models are likely to produce biased estimates (one way or the other) of the survival times of these presences. We return to this issue after presenting the estimated models.

The first step in the analysis is finding an appropriate survival model. A key concept in distinguishing amongst these models is the hazard rate. Allison (1984: 23) defines the hazard rate as the probability of failure in the interval from time T to $T + S$, given that a case has survived to time T (in this case, it is the probability of failure in the interval from minute T to minute $T + 1$, given survival to minute T). Alternatively, Allison states the

³⁷To illustrate, we can look at presences of all lengths (which have not been excluded on other grounds), group them into different length categories, and inspect the percentage of cases in each category which have, say, 30 minutes of follow-up (irrespective of whether a disorder occurred during those 30 minutes) before the end of the observation or the next presence. Using 10 minute duration categories up to 30 minutes and an open-ended category for stops over 30 minutes reveals substantially less potential follow-up time for presences over 20 minutes as compared to presences less than 20 minutes. The percentages of cases having a full 30 minutes of potential follow-up is 11.4% for drive-by presences, 12.8% for 1-10 minute stops, 13.9% for 11-20 minute stops, 6.8% for 21-30 minute stops, and 0% for stops over 30 minutes. (Furthermore, the numbers of cases in the last two categories are only 73 and 54, respectively).

hazard can be described as the unobserved rate at which events occur. Different parametric models make different assumptions about the baseline distribution of the hazard rate, in particular how it changes over time. Construction of life tables (Lee, 1980; Teachman, 1983)³⁸ and plots of the hazard rate against time suggested a log-normal model is appropriate for the data (these are not shown or discussed in detail since our objective is to ascertain the effects of police presences upon survival times).³⁹ The log-normal model assumes the baseline hazard rises and then falls over time. Schmidt and Witte (1988) found the log-normal model (and variations of the log-normal) to be superior to a number of other parametric models in predicting recidivism among two cohorts of prison releasees from North Carolina. In this case, life table estimates of the hazard rate showed that the overall sample (which is dominated by drive-by presences) has a hazard rate that rises, peaks at about five minutes after a presence, and follows a downward trend thereafter.

³⁸The life table is a non-parametric technique often used as a first step in event history analysis. It computes hazard rate values and probabilities of survival for specified points in time. The estimates can be computed for the overall sample and subgroups within the sample. These estimates can suggest what type of parametric distribution (if any) best suits the data.

³⁹In the original sample, this pattern was visible after stratifying presences according to whether they overlapped with any observed disorder. Subsequent hazard plots were constructed both for the final group of selected cases and for all cases which had an observed follow-up period, no ties between the next presence and the next disorder, and no disorder overlap.

These plots also revealed the hazard becomes erratic beginning in the 30-40 minute range. Relatively few cases were observed out to the 30-70 minute range without disorder or censoring (this is especially true for stops over 10 minutes). Consequently, the hazard becomes unstable (particularly within the 40-70 minute range), fluctuating substantially as a result of small numbers of events. For stops overall, single events create dramatic jumps in the hazard at the outer time ranges. Of course, the dissipation, or decay, of residual deterrence may contribute to this fluctuation. There is currently no basis for estimating how long residual deterrence lasts after a presence. Unfortunately, inadequate sample sizes in the stop category (and within subgroups of the stop category) make it difficult to test hypotheses about residual deterrence at the 30-70 minute range. Owing to these problems, the models presented below use a maximum 30 minute follow-up period for each presence. Cases not experiencing an event or censoring prior to 30 minutes are, thus, censored at 30 minutes. Overall, 1210 cases, or 7% of the remaining sample, had disorder failures within 30 minutes. Life table estimates of the survival function indicated the probability of surviving to 30 minutes was 0.85.

The log-normal model is an accelerated failure time event history model which is expressed in terms of the natural logarithms of the survival times:

$$\log T = a + b_1x_1 + b_2x_2 + \dots + b_nx_n + cu$$

where T is the survival time, x_1 through x_n are covariates, and cu is a normally

distributed error term (Allison, 1984; Kalbfleisch and Prentice, 1980).⁴⁰

When using censored data, the model estimates the parameters using maximum likelihood procedures rather than ordinary least squares.

Referring to Table 3, model 1 shows the effect of drive-by presences relative to all stops and the effect of increasing duration for stops (duration is set to zero for drive-by presences). Relative to all stops, drive-by presences actually have significantly longer log survival times. This is probably because, even after removing presences overlapping with visible disorder, many of the stops are responses to problems and take place during times that have higher risk for disorder. Overall, we would expect stops to be a more heterogeneous group than drive-bys. It is also possible that troublesome persons feel more secure after a police stop because they think police are unlikely to return any time soon. However, the duration variable shows that each extra minute of police presence has a significant and positive effect as well.

The coefficients of model 1 show the additive effects of the covariates upon the log of survival time. By performing a simple transformation upon these coefficients, we can ascertain the impact of the covariates upon survival time itself. These effects are multiplicative rather than additive. (When the log-normal model is expressed in terms of survival time, it becomes a multiplicative model showing the multiplicative effects which the covariates

⁴⁰The log-normal model assumes the log survival times are normally distributed (Lawless, 1982: 220).

have upon survival time.) Specifically, we can exponentiate the coefficients in model 1 to see the multiplicative effects of the covariates on survival time. Thus, the drive-by indicator multiplies the mean survival time by $e^{.811} = 2.25$. This indicates that survival times are, on average, 2.25 times longer for drive-bys than for stops. Put another way, the survival times of drive-bys are 125% greater than those of all stops as a group (this percentage change is calculated by subtracting 1 from the multiplicative effect and multiplying the difference by 100). For stops, each extra minute of duration multiplies survival time by $e^{.204} = 1.23$, thereby increasing it 23%.

Model 1 also has a variable for duration-squared in order to test for non-linear effects (i.e., a plateau effect). This variable is not statistically significant, but its negative sign suggests that the benefits of stop duration peak before 20 minutes. We can calculate an estimate of the plateau point by taking the coefficient for duration and dividing it by -2 times the coefficient for duration-squared. This places the plateau point somewhere between 14 and 15 minutes. Figure 1 displays this graphically. Duration increases the log of survival time until duration reaches about 14 minutes. (The total effects on the log of survival time presented on the y axis of Figure 1 are calculated by multiplying each duration value by .204 and adding this to the product of duration-squared and -.007.) Stops appear to be most effective when they are 13-15 minutes long. After 15 minutes, the benefits of increasing duration take

a downturn, though this downturn is not statistically significant.

Both the positive effect of the drive-by category and the possible existence of a maximum utility point for presence length raise two additional questions. First, is there is a threshold point which stops must pass before they create greater residual deterrence than drive-by presences? Second, if there is a point at which stops become superior to drive-bys, is there also a point at which stops cease to be more effective than drive-bys? Drive-bys create a .811 increase in the log of survival time, and Figure 1 shows that stops produce a .8 increase in the log of survival time once they reach about 5 minutes in duration. After stops pass 5 minutes in duration, their effects are always greater than .8. Nonetheless, this does not tell us whether any of these stops are significantly better than drive-bys in terms of residual deterrence.

Model 2 (see Table 3) attempts to address these questions more explicitly. Model 2 breaks presences down into four categories: drive-bys, 1 to 5 minute stops, 6 to 10 minute stops, 11 to 15 minute stops, and 16 to 20 minute stops.⁴¹ The model uses drive-bys as the reference category and tests if the other categories have significantly different effects than the reference group. Relative to drive-by presences, 1 to 5 minute stops have significantly worse survival times, but 11-15 minute presences have significantly better survival

⁴¹The use of five minute intervals is an arbitrary decision, but it seems useful and justifiable. The groupings are small enough to capture meaningful patterns (including the inflection point of the duration response curve in Figure 1), and the sample sizes are still fairly large for most of the categories.

times. The latter presences increase survival time by 388% relative to drive-bys. The other categories have positive but insignificant effects.

Further, the sizes of the coefficients change in the way one would expect based on the results of model 1. As the lengths of the presences become longer, the sizes of the coefficients increase up to a point, after which there is a drop-off. Stops begin to show residual effects superior to those of drive-bys after stops reach about five minutes in length. If a stop is more than 10 minutes, it creates residual effects which represent a very large and statistically significant improvement over the residual deterrence generated by driving through a hot spot. For stops beyond 15 minutes, residual effects decrease and are not significantly better than those generated by drive-bys. However, the small sample size in the 16-20 minute category should make one cautious about drawing strong inferences for that group.

Though the coefficients in these models show that stops of certain lengths are superior to drive-bys, they do not make it entirely clear how much extra residual deterrence these stops generate in terms of minutes. To better illustrate what some of these coefficients mean in terms of real survival times, log-normal survival estimates were computed for follow-up periods of up to 30 minutes for drive-bys and 11-15 minute stops. These estimates form survival curves which are presented in Figure 2. These curves show the probability of surviving without disorder for various follow-up times up to 30 minutes.

Table 4 also shows the probability of survival to selected time points for drive-bys and 11-15 minute stops.⁴²

To illustrate, the probability of having 30 minutes of order following a drive-by is .84. For 11-15 minute stops, this probability is .96. Thus, a sample of 100 drive-bys with 30 minute follow-up periods would be expected to have 16 disorder failures. In contrast, a sample of 100 11-15 minute stops with 30 minute follow-up periods would be expected to have 4 disorder failures.

However, these figures may underestimate the true difference in residual deterrence generated by the two categories. Both Table 4 and Figure 2 show that the difference in survival times between the groups gets larger over time. If the follow-up periods were longer, the residual benefits of 11-15 minute stops would likely appear even greater.

Taken together, the survival models suggest there is a threshold point for the duration of police stops and a possible maximum utility point for stop duration. Using 5 minute duration groupings, it appears stops must last beyond 10 minutes in order to generate significantly more residual deterrence than do drive-by presences. Models 1 and 2 both identify a maximum utility point for duration somewhere in the 11 to 15 minute range. The results are

⁴²These estimates equal 1 minus the value of the cumulative normal distribution evaluated at $(\log T - z)/c$ where T is the chosen follow-up time, z represents the difference between the two categories (drive-bys and 11-15 minute stops), and c is the scale term shown in model 2 of Table 3 (Lawless, 1982: 24). Referring to model 2, m is 5.490 (the constant term) for drive-bys and $(5.490 + 1.584)$ for 11-15 minute stops.

not as clear for stops over 15 minutes, but both models suggest the benefits of longer duration decline after 15 minutes.⁴³ While interpreting these results, we must also keep in mind that they demonstrate the effects of police dosage levels conditional upon there having been no observable disorder during the presence or at the end of the presence (i.e., conditional on an orderly environment).⁴⁴

⁴³These results persisted in more complicated versions of the models which included variables for warm/cold weather months (with May through September coded as warm months), hour of day, and hour of day squared. The warm months had highly significant negative effects on survival time in each model, while the hour of day variables were marginally significant in each run. Nevertheless, introduction of these variables produced almost no changes in the effects of the police variables. The slight changes which did occur were more in favor of stops: drive-bys became only marginally significant ($p=.06$) in model 1 and the negative effect of 1 to 5 minute stops in model 3 became only marginally significant ($p=.07$). Because these variables had very little impact on the police variables, they are not included with the presented models.

⁴⁴The models were also estimated after including those presences which had disorder during one or more minutes of the presence. A dummy variable was used to indicate whether or not there was disorder during the presence. This variable had a very strong and highly significant negative effect on survival time in both models. Otherwise, the results were similar to the results presented in the text. The drive-by and duration variables were both significant in model 1, though the drive-by indicator did not have as large an effect. Duration-squared was again negative and insignificant, suggesting a possible plateau point around 18 minutes.

Model 2 showed more substantial changes, but the basic pattern of the results remained the same. The coefficients of the duration categories increased, peaked at the 11-15 minute category (which generated a 265% increase in survival time relative to drive-bys), and decreased for the final category. However, the negative effect of the 1-5 minute category lost its significance. Further, the 6-10 minute category produced a statistically significant ($p=.05$) 53% increase in survival time relative to drive-bys, while the 16-20 minute category produced a marginally significant ($p=.08$) 104%

A limitation of these models is that they take an ahistorical view of each presence and its aftermath. That is, previous events (presences and disorders) both within each 70 minute observation window and over the course of the entire year could affect each presence and its associated survival time. Yet, the randomly selected starting times and relatively short lengths of the observations make it difficult to reconstruct even the most immediate history of the location. Moreover, there is no theoretical basis upon which to decide how far back in time such measures should go. Considering the flow of activity and people into and out of hot spots (Sherman et al., 1989), very recent history (i.e., the last hour) may matter very little. Besides, examining the last few minutes before a presence is likely to be an incomplete consideration of prior history. The most important effects of different levels of police presence may come from patterns developing over several hours, days, months, or years. Capturing these effects would require longitudinal study of the "criminal careers" of the hot spots. In an attempt to test for any cumulative effects from the experiment, a dummy variable indicating whether the hot spot was an experimental or control hot spot was entered into the above models. The variable did not have a significant impact and was omitted from the models (including this variable also had no substantial

increase in survival time relative to drive-bys. Thus, the impact of stops may be somewhat stronger when the environment of the hot spot is disorderly. This suggests the findings presented in the text may be conservative estimates of the effects of police dosage.

impact on the effects of the police variables).⁴⁵ More complicated efforts to model historical factors is beyond the scope of this investigation. Nonetheless, the ability of the models to capture duration effects despite this problem is impressive.

A related issue is that the models do not have variables representing individual characteristics of the hot spots.⁴⁶ This problem should be minimized, however, by the selection process by which the locations were chosen. As discussed previously, all of the locations had to generate minimum numbers of hard and soft crime calls within a one year period, show reasonable stability in hard crime calls over a two year period, and be public locations or locations with spillover of activity into public space.

To test the robustness of the results and the appropriateness of the log-normal distribution, the same models were estimated using different parametric and semi-parametric models. Some of the most commonly used parametric distributions are the exponential and Weibull distributions. Exponential models assume a constant hazard rate while Weibull models

⁴⁵In spite of more frequent presences in the experimental locations, the average potential follow-up time for presences in the experimental areas was only 1 minute less than that for control area presences. It seems unlikely, therefore, that the insignificance of the experimental/control variable is due to systematically shorter censoring times for presences in the experimental areas.

⁴⁶Call data for the hot spots are not included in the observational data and were not available to the author.

assume the hazard rate either decreases or increases monotonically over time. In other words, Weibull models assume a hazard that either increases or decreases at a constant rate; it does not change direction.

The exponential model is a special case of the Weibull model. Basically, it is a Weibull model with a restriction that the hazard rate be constant. This restriction can be formally tested using log likelihood ratio tests. These tests (not shown) indicated the restriction was reasonable for each model. Thus, the Weibull distribution was inappropriate for the data.

The exponential model can be expressed as an accelerated failure time model using the same equation as that presented for the log-normal model. The only difference is that the exponential model assumes the error term follows an exponential distribution rather than a normal distribution. Table 5 shows that estimating the models with an exponential distribution produced results which are essentially the same as those obtained with the log-normal distribution in terms of directions, statistical inferences, and magnitudes of effects. Though the exponential and log-normal models cannot be formally tested against one another, inspection of the log likelihoods produced by each model provides an informal assessment of which model has a better fit to the data (Schmidt and Witte, 1988). The log-normal models produced log likelihoods that were 50 to 55 points better (closer to 0) in each instance.

Finally, the results were tested using the general proportional hazards

model developed by Cox (1972). This model, often referred to as a semi-parametric model, makes no assumptions about the distribution of the baseline hazard. In essence, it estimates the impact of the covariates upon the hazard rate without presenting an estimate of the baseline hazard rate. The Cox models are not presented here because they use a different equation than the accelerated failure time models. Therefore, the coefficients of the Cox models and the accelerated failure time models are not equivalent without use of a transformation procedure. Suffice it to say the Cox model concurred with the log-normal and exponential results in terms of directions of effects and statistical inferences.

The discovery of a log-normal distribution for this application is itself interesting and worthy of discussion. As stated previously, the hazard plots revealed the likelihood of a new disorder is greatest about five minutes following a presence (though even at this point the life table estimate of the hazard was only about 0.01). A possible interpretation is that the occurrence of disorder following a police presence is more a function of people remaining around the hot spots than it is a function of the flow of people into and out of the hot spots. If police do not drive troublesome persons away from a hot spot, those persons are most likely to engage in (or resume) some kind of disorderly activity a few minutes after police leave. During those first few minutes following a presence, these people may think police are unlikely

to arrive again very soon. The hazard rate may take a few minutes to peak because police presence makes people temporarily more cautious (as hypothesized earlier) if they remain at the hot spots. The log-normal survival models indicate that, up to a certain point, stronger dosages of presence decelerate the time to failure, pushing back the peak time for disorder (Kalbfleisch and Prentice, 1980: 34). Longer presences most likely accomplish this by both driving away troublesome persons and creating a longer cautionary period among those who are still around.

Before concluding the survival analysis, it seems desirable to attempt to provide some further indication as to whether the effects of duration have a real plateau at 15 minutes and whether police presences over 20 minutes improve residual deterrence. Although the data have built-in problems which make it difficult to assess the impact of presences over 20 minutes, an informal examination may be helpful. Table 6 uses stops of up to 30 minutes, breaks them into 10 minute categories, and presents the percentage failing within a designated time, x , given that the cases had x minutes of follow-up without censoring. For example, the top row presents the percentage of cases in each category failing within the first ten minutes of follow-up given the cases had at least ten minutes before the occurrence of new police presences or the end of their respective observations. The next rows repeat this procedure for 20 and 30 minute follow-ups, though the number of cases in the

21-30 minute presence category becomes very small (beyond 30 minute presences, the number of cases with even 10 minutes of uncensored follow-up is too small to be useful). This table suggests that with a minimum ten minute follow-up period, the 21-30 minute presences do well. After this, the number of cases followed for 20 and 30 minutes is too small to really be useful. Unfortunately, the data do not allow us to draw any firm conclusions about presences over 20 minutes. Based on the available data, it appears that, within a 20 minute duration range, the maximum utility point for presence duration is between 14 and 15 minutes.

As a final note, survival analyses were conducted only for overall disorders. Though it was desired to run the models for criminal events only, the number of cases with criminal failures is so small (around 1% of the cases) it would be very difficult to find any significant effects (Allison, 1984: 50).

VI. DISCUSSION AND CONCLUSIONS

This study has yielded a number of insights into the deterrent effects of police patrol. Specifically, the observational data demonstrate both direct and residual deterrence from patrol. Analysis of direct deterrence showed immediately visible police presence in a small geographic area has a direct deterrent effect on the outbreak of both disorderly and criminal conduct in that area. However, direct deterrence was primarily generated by presences in the experimental hot spots. This effect may have been due to a lower proportion of reactive presences among this group, a cumulative process whereby proactive presences had a reinforcing effect on one another, or a combination of the two. At any rate, it appears proactive police presence at a hot spot raises perceptions of risk and perceptions of the certainty of punishment or legal intervention for criminal and disorderly persons. Moreover, this effect is greater on criminal acts. Compared to the other behaviors recorded by the observers, these acts carry greater certainty and severity of punishment. The behaviors which are the most deterrable in theory turned out to be the most deterrable in these data.

The investigation also clearly demonstrated that enhancing patrol levels in small areas creates residual deterrence that decreases crime and disorder by substantial amounts during times when police are not visibly present. This

concept has not been explicitly tested by other patrol studies. Indeed, the increases in patrol levels at the experimental locations produced residual reductions in measures of both crime and overall disorder that were greater than the direct reductions achieved during specific instances when police were present in the control areas. The findings of residual deterrence raise the possibility that residual deterrence produced by proactive presences in the experimental areas spilled over into experimental police minutes, making direct deterrence appear greater for these presences. The data are consistent with, although they do not measure, the hypothesis that enhanced patrol levels changed offenders' perceptions about the experimental hot spots by making these hot spots seem less hospitable to disorderly and criminal conduct. This finding thus fails to falsify a fundamental claim made by many deterrence scholars.

Investigation of residual effects associated with specific instances of police presence showed stronger dosages, as measured by the durations of the police presences, improved residual deterrence. Each extra minute of presence at a hot spot created a statistically significant increase in the time until the next occurrence of crime or disorder (criminal acts could not be examined separately for this portion of the analysis). Yet, these results also suggest there is a threshold point which dosage must reach before it generates significantly more residual deterrence than that generated by simply driving

through a hot spot. Analysis with survival models suggests this threshold is reached at about 10 minutes. When police stop at a hot spot for over 10 minutes, they create a large and statistically significant increase in the time until the first disorder after they depart. The data also show that, within a 20 minute dosage range, the benefits of dosage peak at 14-15 minutes and decrease thereafter. However, limitations of the data and the insignificance of the non-linear effect in model 1 should temper conclusions regarding this leveling off point.

The findings imply that longer presences, at least up to a point, heighten uncertainty and raise perceptions of risk at hot spots. This lengthens survival times, probably through a combination of driving some troublesome persons away and making others more cautious for some time afterwards. In fact, the implementation of longer stops at the experimental hot spots may have been a primary mechanism by which crime and other disorderly behaviors were reduced in those hot spots. Taken together, the results of the minute-based and survival analyses suggest police can maximize their deterrent effect at hot spots by implementing proactive, medium-length stops at these locations on a random, intermittent basis. This way, police can maximize residual deterrence and perhaps minimize the amount of unnecessary time they spend at hot spots (i.e., instances when residual deterrence is spilling over into police-present time).

An important finding implicit within the aforementioned results is that preventive patrol decreases non-criminal disorder. Despite the difficulties inherent in deterring these behaviors (i.e., value conflicts, emotional contexts, and lack of criminal penalties), higher patrol levels and longer presences both exhibit a restraining effect. This study also provides further evidence of the link between disorder and crime. These results and those of Sherman and Weisburd (1990) reveal patrol reduced both crime and non-criminal disorder. Although these studies cannot conclusively demonstrate that reduction of non-criminal disorder is a causal mechanism for crime reduction, the studies do provide clear indications these phenomena are intertwined. At a minimum, we can say they are responsive to the same preventive measures (i.e., police presence in cars and/or uniform). This could be interpreted as additional support for Wilson and Kelling's (1982) incivilities thesis, though not at the neighborhood level.

A number of policy implications emerge from these results as well. This study reinforces Sherman and Weisburd's (1990) contention that preventive patrol, if properly focused, has a deterrent (or at least a displacement) effect on crime. More specifically, it provides further justification for proactively concentrating more patrol resources on specific troublesome locations. Though this would entail more emphasis on locations as opposed to neighborhoods, there is empirical and anecdotal evidence justifying such an

approach. Commenting on the substantial variation of predatory crime within communities as well as across communities, Sherman et al. (1989: 43) state, "...the general safety of places in the city's 'dangerous' neighborhoods further suggests the theoretical and policy significance of the criminology of place as distinct from the traditional criminology of neighborhoods or areas." Pierce et al. (1988) recommend directing proactive enforcement against recurring, predictable problems at troublesome locations. Similarly, Weisburd and McElroy (1988: 92,95) report foot-based, community patrol officers operating in one of New York City's precincts often focus their efforts on crime and disorder problems at particular locations or blocks within their beats. Finally, Taylor and Gottfredson (1986) review evidence concerning physical environment and crime prevention and conclude crime prevention efforts should focus on particular street blocks rather than neighborhoods.

The basic relationships demonstrated by these analyses indicate police departments can productively increase patrol levels around hot spots and implement random proactive stops at these locations. It appears such stops should last between ten and fifteen minutes in order to maximize their effectiveness. Police departments could conceivably target many hot spots at once by assigning patrol officers to a number of hot spots and having officers make regular stops at each of their assigned spots.

Still, there are a number of questions remaining about hot spot patrol

interventions. Most obviously, this study has not addressed what officers do while at hot spots. These results have provided a basis for proactive presence at such locations. A logical next step (besides that of further investigating the maximum utility point for stop duration) would be to experiment with different styles of policing at hot spots. Departments might have officers conduct short walking tours of hot spots or engage in problem oriented policing (Eck and Spelman, 1987; Goldstein, 1979; Sherman, 1986) at hot spots. Giving officers substantial autonomy in their handling of hot spots might well enhance their job satisfaction and enthusiasm for policing hot spots.

Departments could also set higher priority on disorder enforcement at these locations. If, on the other hand, preventive patrol can deter disorder without resort to aggressive tactics (as this study has suggested), police may choose to adopt a posture that is not overly aggressive. Although this study has shown that preventive patrol can prevent disorder, it does not address how officers should best handle disorder (particularly non-criminal disorder) that does occur when they are present.

Another question concerns displacement. The data do not permit assessment of displacement effects.⁴⁷ Nevertheless, hot spots seem to have social and physical characteristics facilitating crime and disorder. As stated

⁴⁷Preliminary results offered by Sherman and Weisburd (1990) have not yet examined the displacement hypothesis.

earlier, any displacement that does occur may displace disorder to areas not as conducive to such conduct. Besides, departments could monitor this on an ongoing basis. A department could shift targets if and when it discovered indications of crime and disorder displacement.

Further experimentation is needed to determine whether location-oriented patrols would be successful in other cities with warmer climates and greater crime problems than those of Minneapolis. In addition, future studies may be able to show how often officers should stop at hot spots. The major implication of this study is that increased patrol levels and stronger dosages of presence can reduce crime and otherwise disturbing behavior at a city's most troublesome locations, thereby enhancing the effectiveness of preventive patrol.

Table 1. Minute-Based Disorder Initiation Comparisons: Proportions of Minutes With Disorder Initiations

Group 1 Minutes	Group 2 Minutes	Relative Difference	Z score ^a
Non-police (all) 3304/379622 = .00870	Police (all) 209/30372 = .00688	-21%	-3.31***
Non-police (ctrl) 1891/199034 = .00950	Police (ctrl) 65/7261 = .00895	-6%	-0.48
Non-police (ctrl) 1891/199034 = .00950	Police (exper) 144/23111 = .00623	-34%	-4.94***
Non-police (ctrl) 1891/199034 = .00950	Non-police (exper) 1413/180588 = .00782	-18%	-5.57***

*** significant at $p \leq .001$

^aSignificance tests are 1-tailed. For each comparison, the proportion of cases having disorder initiations is hypothesized to be smaller in the second group. Proportions were tested for equivalence after rounding to five decimal places.

Table 2. Minute-Based Crime Initiation Comparisons: Proportions of Minutes With Crime Initiations

Group 1 Minutes	Group 2 Minutes	Relative Difference	Z score ^a
Non-police (all) 352/379622 = .00093	Police (all) 19/30372 = .00063	-32%	-1.67*
Non-police (ctrl) 228/199034 = .00115	Police (ctrl) 7/7261 = .00096	-17%	-0.47
Non-police (ctrl) 228/199034 = .00115	Police (exper) 12/23111 = .00052	-55%	-2.75**
Non-police (ctrl) 228/199034 = .00115	Non-police (exper) 124/180588 = .00069	-40%	-4.64***

* significant at $p \leq .05$

** significant at $p \leq .01$

*** significant at $p \leq .001$

^aSignificance tests are 1-tailed. In each comparison, the proportion of cases having disorder initiations is hypothesized to be smaller in the second group. Proportions were tested for equivalence after rounding to five decimal places.

Table 3. Log-Normal Survival Models (N=16,997)

Models	B	Std Err	Chi-square	Grp N
Model 1				
ln L = -5244.5				
drive-by	.811	.342	5.622*	
duration	.204	.102	4.005*	
duration ²	-.007	.006	1.714	
constant	5.021			
scale	2.11			
Model 2				
ln L = -5240.7				
stop 1-5 mins	-.343	.173	3.947*	481
stop 6-10 mins	.286	.268	1.135	279
stop 11-15 mins	1.584	.619	6.548**	121
stop 16-20 mins	.058	.486	.014	66
constant	5.490			
scale	2.109			

* significant at .05 level

** significant at .01 level

For the interested reader, the constant (a in the equation on page 61) represents the -log of the hazard rate. The scale term (c in the equation on page 61) represents p^{-1} where p is a parameter affecting the scaling of the distribution of logT and, hence, the shape of the hazard rate. See Kalbfleisch and Prentice (1980).

Table 4. Log-Normal Survival Estimates for Drive-bys and 11-15 Minute Stops

Follow-Up Time	Probability of Survival to Time T (Drive-bys)	Probability of Survival to Time T (11-15 Min. Stops)
5 minutes	.967	.995
10 minutes	.935	.988
15 minutes	.906	.981
20 minutes	.881	.973
25 minutes	.859	.966
30 minutes	.839	.959

Table 5. Exponential Survival Models (N=16,997)

Models	B	Std Err	Chi-square	Grp N
Model 1				
ln L = -5297.2				
drive-by	.772	.290	7.066**	
duration	.181	.092	3.894*	
duration ²	-.006	.005	1.328	
constant	4.306			
scale ^a	1.000			
Model 2				
ln L = -5294.2				
stop 1-5 mins	-.367	.147	6.190**	481
stop 6-10 mins	.270	.260	1.078	279
stop 11-15 mins	1.575	.708	4.954*	121
stop 16-20 mins	.085	.448	.036	66
constant	5.078			
scale ^a	1.000			

* significant at .05 level

** significant at .01 level

^aThe exponential model restricts the scale term to equal one.

Table 6. Percentages of Cases Across Dosage Categories Having Disorder Failures Within Follow-Up Time X, Given Uncensored Follow-Up To Time X.

Uncensored Follow-Up	Drive-bys	1-10 Min. Stops	11-20 Min. Stops	21-30 Min. Stops
10 Minutes	7.2% (567/7829)	8.4% (31/370)	5.1% (5/99)	0% (0/34)
20 Minutes	11.7% (430/3685)	14.2% (24/169)	4.1% (2/49)	5.9% (1/17)
30 Minutes	14.1% (258/1827)	19.6% (19/97)	3.8% (1/26)	0% (0/5)

Figure 1. Duration Response Curve

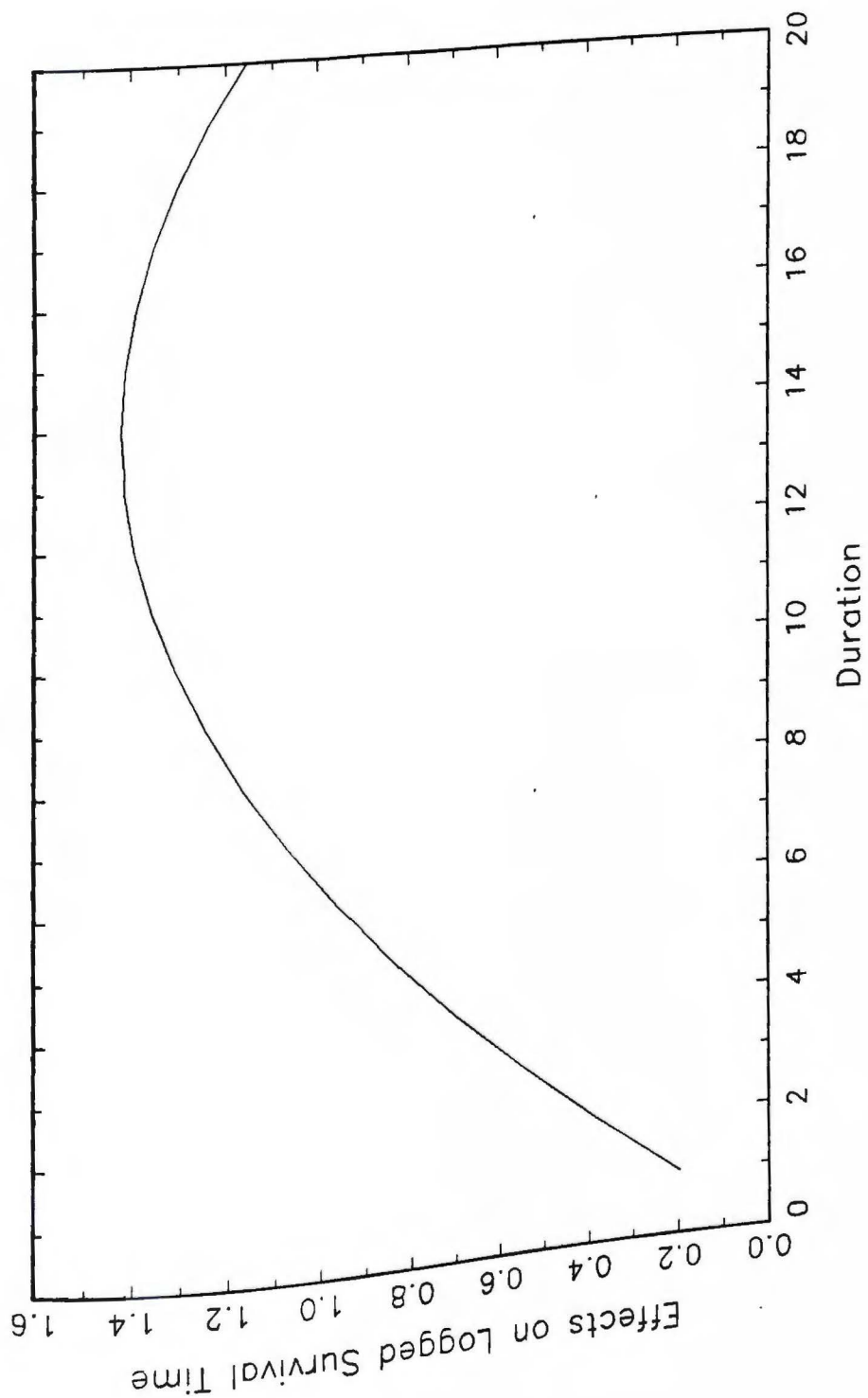
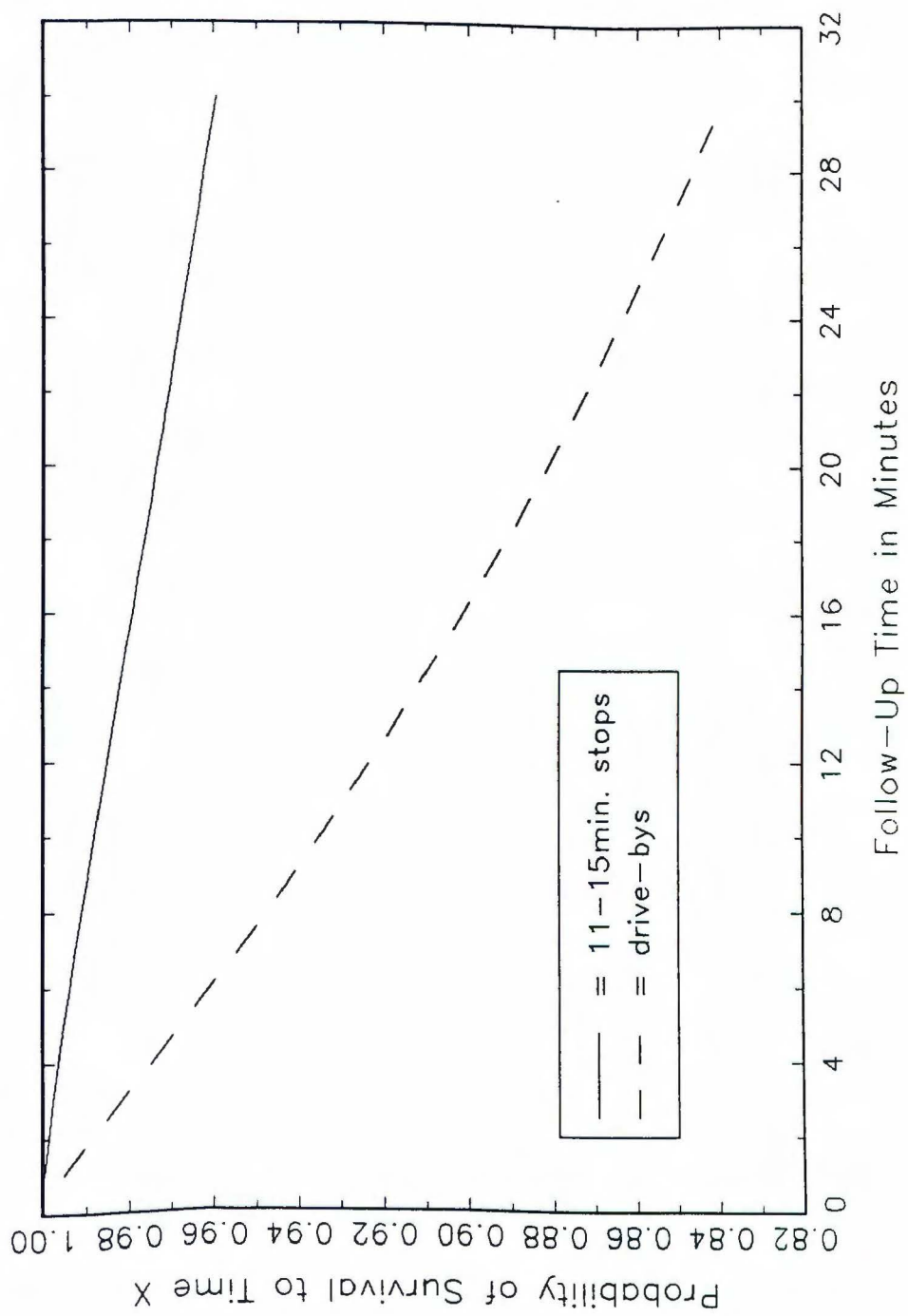


Fig 2. Log-Normal Survival Curves for Drive-bys and 11-15 Min. Stops



APPENDIX A. DESCRIPTIVE STATISTICS

Variable	Mean	Std Dev	Min	Max
Observations per Hot Spot	62.73	6.98	46	83
Observed Minutes per Hot Spot	4452.65	495.07	3266	5893
Disorders per Hot Spot	40.14	23.80	5	114
Police Presences per Hot Spot	194.98	99.01	39	528
Minutes per Observation	70.98	1.25	20	102
Disorders per Observation	0.64	1.60	0	35
Police Presences per Observation	3.11	2.45	0	15
Disorder Length (all)	3.95	8.25	1	76
Crime Length	6.18	11.51	1	76
Non-drive-by Police Presences (all blocks)	15.33	16.27	1	71

Variable	Experimental Hot Spots	Control Hot Spots
Observations	3135 50%	3138 50%
All Disorders	1798 44.8%	2216 55.2%
Crimes	153 36.6%	265 63.4%
Police Presences	9068 53.4%	7929 46.6%
Drive-bys	8505 53%	7545 47%
All stops	563 59.5%	384 40.5%
Stops 1-5 mins	262 54.5%	219 45.5%
Stops 6-10 mins	163 58.4%	116 41.6%
Stops 11-15 mins	88 72.7%	33 27.3%
Stops 16-20 mins	50 75.8%	16 24.2%

Disorders per Observation	Frequencies	Percent
0	4473	71.3
1	958	15.3
2	395	6.3
3	170	2.7
4	98	1.6
5	57	.9
6	37	.6
7	27	.4
8	11	.2
9	14	.2
10	6	.1
11	7	.1
12	5	.1
13	3	.0
14	2	.0
15	3	.0
16	1	.0
17	2	.0
19	1	.0
20	1	.0
21	1	.0
35	1	.0

Disorder Type	Number Observed
Verbal Disorders	796
Loud Disputes	204
Drunk or Drugged Behavior	652
Loud Noise/Music	1623
Bag Persons	113
Persons Down	38
Solicitation	119
Drug Activity	201
Physical Assaults	62
Auto Break-ins	4
Building Break-ins	1
Vandalism	35
"Other" Disorders	255
Unidentified Disorders	45

* These counts do not sum to 4,014 because some instances of disorder involved more than one act.

Presences per Observation (all blocks)	Frequencies	Percent
0	708	11.3
1	1135	18.1
2	1161	18.5
3	998	15.9
4	767	12.2
5	525	8.4
6	358	5.7
7	249	4.0
8	136	2.2
9	111	1.8
10	54	.9
11	37	.6
12	22	.4
13	7	.1
14	1	.0
15	4	.1

SURVIVAL DATA SUMMARY

Time in Minutes	Number At Risk	Disorder	Censored
1	16997	133	232
2	16632	123	1654
3	14855	122	1314
4	13419	99	1094
5	12226	130	1245
6	10851	67	846
7	9938	53	792
8	9093	58	675
9	8360	42	578
10	7740	45	711
11	6984	32	504
12	6448	34	462
13	5952	45	397
14	5510	27	339
15	5144	31	464
16	4649	26	317
17	4306	13	292
18	4001	14	264
19	3723	11	241
20	3471	24	274
21	3173	17	178
22	2978	6	183
23	2789	7	205
24	2577	7	166
25	2404	10	195
26	2199	10	138
27	2051	7	113
28	1931	8	133
29	1790	4	109
30	1677	5	1672

Variable	Mean	Std Dev	Min	Max
Duration of Police Stops Used for Survival Analysis	6.55	4.69	1	20
Time to Disorder or Censoring (all presences)	11.41	9.07	1	30
Time to Disorder or Censoring (drive-bys)	11.40	9.07	1	30
Time to Disorder or Censoring (all stops)	11.53	9.11	1	30
Time to Disorder or Censoring (stops 1-5 mins)	11.10	8.94	1	30
Time to Disorder or Censoring (stops 6-10 mins)	11.30	9.06	1	30
Time to Disorder or Censoring (stops 11-15 mins)	12.81	9.49	1	30
Time to Disorder or Censoring (stops 16-20 mins)	13.24	9.58	1	30

Variable	Mean	Std Dev	Min	Max
Potential Follow-Up (all presences)	12.13	9.32	1	30
Potential Follow-Up (drive-bys)	12.12	9.32	1	30
Potential Follow-Up (all stops)	12.27	9.41	1	30
Potential Follow-Up (stops 1-5 mins)	12.02	9.39	1	30
Potential Follow-Up (stops 6-10 mins)	12.03	9.35	1	30
Potential Follow-Up (stops 11-15 mins)	12.98	9.52	1	30
Potential Follow-Up (stops 16-20 mins)	13.89	9.60	1	30

APPENDIX B: OBSERVER CODEBOOK

CODEBOOK FOR HOTSPOT OBSERVATION;
POLICE PRESENCE AND STREET DISORDER

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Compiled November 15, 1988
Updated June 20, 1989

CODE BOOK: POLICE PRESENCE

1. **Date:** The date of the observation in 6-digit code, month-day-year, (February 21, 1989, is written 02-21-89 or 02/21/89).

2. **Time In:** The time that the observer actually begins observation at the assigned hotspot (after parking, getting the checklist ready, etc. Use a standard 12-hour time. All observations will be sometime between 6:30 p.m. and 2:30 a.m., so it is not necessary to write "pm" or "am" in the space.

3. **Time Out:** The time (in 12-hour notation) that the observer completes the observation, prior to moving from the hotspot.

Note: It is vital that all observation times be recorded exactly. DO NOT "FUDGE IT" -- If you are delayed, record the actual time you begin your observations. If you are late beginning, stay later at the end, so that the observation period is always 70 minutes.

Because we are doing random sampling, the time that the observation begins is less important than the length of the observation. However, because we must conduct random checks on your presence at each spot, recording the actual time you are there is vital to the verification process. The verification process, in turn, is vital to the credibility of the project.

If you are delayed for more than ten minutes, call the supervisor.

4. **Hotspot #:** This number will be entered on your sheets before you go out. Check your sheets before you go out. If the number is not there, ask the supervisor.

5. **a. Observer Code:** On the first part of the line write the observer number that you have been assigned.

b. Observer Sequence Code: The four digit number after the dash should be filled in before you receive the sheets. If it is not, please ask the supervisor.

6. **Position:** Note which parking spot you were in (from the numbered dots marked on the maps). If you were not parked within one car length of the identified spots, mark a 0.

7. **Parallel Coded:** Leave this blank for the supervisor to complete. (Note: 1 = yes; 2 = no.)

8. **Drive by:** Leave this blank for the supervisor to complete.
(Note: 0 = drive-by not done;
1 = observer was present for the drive-by;
2 = observer was not present for the drive-by.)

Population Estimates: At the bottom of the sheet, there are population estimates to be completed at the beginning and at the end of the shift.

Time: Record the time that you counted the cars. This should be within five minutes of the beginning of the shift for the first estimates, and within five minutes the end of the shift for the second estimates. It is not necessary that these be done at the exact minute the shift begins and ends.

a) Number of People Out-of-doors: Count the number of people who are visible out-of-doors within the hotspot boundaries and enter this number in the blank. Include people on porches.

b) Number of Parked Cars: Count the number of parked cars within the hotspot. Include any cars in parking lots if the parking lots are within the hotspot boundaries. Do not count cars in car dealer lots or used car lots.

c) Number of Stopped Cars: Count the number of cars that drive through the hotspot, in any direction, within a fifteen second time period.

Note: Make sure items 1 - 8 are filled in completely and accurately and that the "police presence" sheet matches relevant items with the "disorder" sheet for each hotspot observed.

TERMS

A Squad is Minneapolis police language for a marked police car. For our purposes, the term denotes only the car, not the individual officers in the cars. For our purposes, any uniformed police officer in a marked squad--whether Minneapolis Police, Park Police, University Police, Sheriff's Department, State Patrol, Housing Patrol, or suburban police department passing through--counts as a squad.

If uniformed officers get out of an unmarked car, they are coded as Foot Officers. Since their deterrent effect resides in their visible uniforms, and not in the unmarked car, you should record the officers' presence, and not the car's.

A Foot Officer is any uniformed officer who walks in the public space of the hotspot, regardless of whether they arrive on foot or in a squad car. If the officer arrives in a squad, gets out of the car and goes directly into a building (as though answering a call), it is not a "foot patrol" but an "enters building" category (see below). It should be recorded how the officer came into your view, the "arrived in squad" or "arrived on foot" column should be checked along with the appropriate activity, such as foot patrol.

If two officers walk together, it is one foot patrol; only if they separately walk in different areas of the hotspot do you code two foot patrols.

A squad and/or foot officer should always be counted as

"present" when it is within the hotspot boundaries. It may also be counted as "present" if it is just outside of the boundaries, but within vision and may have a deterrent effect. To determine ambiguous squad information, the observer should ask the question, "if I were a mugger, would I be deterred by that officer or squad?" If the answer is yes, then record the presence; if no, don't record anything.

OFFICIAL PRESENCE:

11. Enters

This column records the time you become aware of a police car or a police officer on foot, or "Other Official Presence" (see below, #s 28-33). All observations of squad cars and officers on foot must have an entry in this column (the squad, not the number of officers in it, is the unit of analysis for mobile units).

If an occupied squad is already in the hotspot when the observation period commences, enter the "Time In" time in the "Enters" column, and check the appropriate Squad-Based Activity column to record its activity. Or, if an unoccupied squad is present when the observation period begins, enter the "Time In" time in the "Enters" column, and check the "Unoccupied Squad" column.

Comment: "Unoccupied Squad" (see #17 below) is a default category. It should be used only when you first arrive at a hotspot, and can see only the squad and not the officers.

12. Leaves

This column records either the time that you observe a squad or foot patrol officer(s) or "Other Official Presence" leave the boundaries of the Hot Spot, or any point at which the squad or foot officer(s) leave your field of vision. There are two exceptions to this rule:

a) If you see the officers enter a building within the Hotspot boundaries, begin a new "Activity Begins" entry (see #13, below) on the next line down, and check the "Enter Building" column on that line;

b) If officers who arrived in a squad get out on foot, begin a new "Activity Begins" entry (see #13, below) on the next line down, and check the "Foot Patrol" column on that line. If the officers then leave your field of vision but the squad is still in plain sight, make a notation in the "Time Ends" (#16) column for the "Foot Patrol" observation. We are assuming that the squad will remain visible even though the officers are out of sight, and the effect of the squad will be reflected in the duration of the "Presence" times. The time the unoccupied squad is visible will be reflected in the times in the original "Presence: Enters/Leaves" columns (#11 and #12). See Appendix A, Example #1.

If a squad drives into your field of vision, turns a corner and passes out of your sight, record a "Drive-Through" even if you soon see officers enter your vision on foot from the direction the squad went. Do not assume that they are the same officers, and/or that the squad car is parked within the hotspot boundaries. Make a new "Presence:Enters" entry and check the "Foot Patrol" column.

If a squad and/or foot officer is still within the hotspot when the observation period ends, enter the "Time Out" time in the "Leaves" column on the same line as the "Enters" time.

13) Activity Begins

As indicated above, this column is for recording specific activities (such as foot patrol, citizen contacts, etc.) that occur while a squad or officer(s) is visible in the hotspot. The column will not always be used.

Separate contacts may occur within an official presence; each should be given their own line of the checksheet.

14) Activity Ends

This column records the end of each individual activity noted in #13 above. Record it on the same line as #13.

Squad-Based Activity

15) Unoccupied Squad

"Unoccupied Squad" is a default category. It should be used only when you first arrive at a hotspot, and can see only the squad and not the officers.

16) Code 3

This column is used only for those times when a squad car passes through the hotspot with lights and/or siren in use.

It is not used to note when a squad arrives at the hotspot with its lights and siren, and stops within the boundaries. "Drive through the hotspot" means either "when you see the squad leave the hotspot" or "when the squad leaves your field of vision."

Note Code 3 arrivals in the "Comments" section of the line.

17) Drive-Through

Check this column when you observe a squad drive through the hotspot without stopping, when it has no lights or siren in operation.

Record both the "Enters" and "Leaves" times on the same line, even if they are the same minute.

18) TLE

TLE is the police code for "Traffic Law Enforcement." Here, it indicates any time that you see a squad pull another car over using lights and/or siren. Use this column for any ticketing or towing activity.

If the police arrest someone from the stopped car, indicate that activity on the next line, as a new activity.

19) Accident

If an accident occurs within the hotspot (or has occurred prior to your arrival), each squad that arrives at the scene should have a separate "Police Presence" entry, with a check in this column. The main purpose of this is to track the number of squads in the hotspot at any given time. It is not necessary for you to match the arrival and departure time of each squad. All that is necessary is to note arrivals and departures in order.

20) Stopped in Hotspot

This column applies to those times when a squad pulls in to the hotspot and the officer(s) inside remain(s) seated, possibly writing a report, or just monitoring the activity in the area.

Check this column if, after seeing a squad pull in and park, you do not see the officers get out within a minute. The time of entry will be recorded in the "Police Presence: Enters" column (#11), and this column will be checked.

If the officers subsequently begin a foot patrol after being in the car for a while, or go into a building, drop down a line and check the appropriate activity, noting the time in the "Activity Begins" column (#13). See Appendix A, Example #2.

Foot-Based Activity

The activities here are not listed in any particular order or priority; some or all activities can be done in sequence. "Foot Patrol" may include several "Citizen Contacts"; it may precede or follow "Enter Building," etc.

21) Arrived in Squad

Check this box if you see officers arrive in a marked squad.

22) Arrived on Foot

Check this box if you see officers enter the hotspot on foot, or if uniformed officers get out of an unmarked car.

23) Foot Patrol

This category should capture a deliberate walking tour of the hotspot by one or more officers. If you see no squad car, but do observe an officer walking in the hotspot, check this column and note the time in Column #11, "Presence: Enter."

Do not check this column if a squad pulls up, the officers get out, and immediately enter a building, or make contact with a citizen on the street. Check the appropriate column, since it appears that the police are there for the purpose of answering a call or checking up on a person.

24) Enter Building

Check this column whenever the officers leave your field of vision by entering a building. The entrance remains within your

field of vision, and in most cases you will be able to tell when the officers leave.

If a second car pulls up and the officers enter the building, too, begin a new "Presence" line, and check this column on that line.

If an officer appears on foot and enters the building, make a new "Presence" entry and check "Enters Building" and "Arrived on Foot" on the same line. For our purposes, it does not matter whether the police appear to be there on business, or on a meal or coffee break. See Appendix A, Example #3.

25) **Verbal Citizen Contact**

Begin a new "Activity" line and check this column whenever you see an officer or officers approach a citizen or group of citizens on the street, and no physical activity (such as a frisk, a shoving match, or an arrest) is observed.

Comment: We will not always be able to distinguish cordial from hostile contacts, especially at a distance, so we are using the broad categories only.

Comment: The contact is the unit of analysis, not the number of persons or officers involved in the contact. If a team of officers approach a single person or a group of people, count it as one contact. Splitting up two disputants, with one officer talking with each party, is still a single contact. Multiple "Activity" entries should be made only when the officers very clearly split up and make contact with different and unassociated persons on the street, or when the officers make contact with the same group at different times.

Comment: If officers in a squad appear to have a verbal contact with citizens from the car (i.e., the officers do not get out of the squad), code the time as "Verbal Citizen Contact."

26) **Physical Citizen Contact**

Any contact between officers and citizens where you can observe physical contact -- a pat frisk by the officers, a shoving match, or the use of weapons, regardless of whether the citizen in question is taken into custody -- should be checked as a separate "Activity Duration" line.

"Physical Contact" also includes pointing a gun at a person, and tapping or prodding them with a nightstick or flashlight.

27) **Arrest**

Whether or not an arrest is/will be made will not always be clear to the observer at first. If you see the police pull up, get out of the squad, and take someone into custody almost immediately, check only this column.

If the police make a citizen contact of several minutes' duration, and then take someone away in handcuffs--or if there is a physical confrontation, with a similar result--you may have checked one or the other already. Even though the arrest was the conclusion of a particular citizen contact, the arrest should be

recorded on a separate "Activity" line. The time that you determine that an arrest has been made should end the preceding activity line, and begin a new activity line with this column checked. Each squad that arrives at the scene should have a separate "Arrest" entry. See Appendix A, Example #4.

OTHER OFFICIAL PRESENCE

28) Security Guard

If a uniformed private security guard is visible to you (and guards who work inside occasionally check the outside of their buildings), note the time that guard is visible on a separate "Presence" line.

The activities listed are for uniformed police only. If the official presence is a security guard, record only the "Enters" and "Leaves" time, and record in the "Comments" any activities the security guard may do.

Note: Minnesota state law forbids security guards to wear blue, green, brown or maroon uniforms (police, Department of Natural Resources, Sheriff's Department, and State Patrol colors, respectively).

Off-duty police officers work as security at many commercial establishments, usually in uniform. We are most concerned with the presence of the police, not with who is paying them. If you think the officer is working off-duty, record that observation in the "Comments" column, but otherwise code that officer's presence and/or activities as if he or she were a foot patrol officer.

29) Security Vehicle

Check this column if a marked security car (i.e., with roof lights and/or side markings) drives through the hotspot or is observed parked there. This category also includes persons in uniform on scooters (funeral escorts), animal control vehicles, armored cars, meter monitors, etc.

30) Parked Fire Dept. Equipment

If you see a fire engine (or fire chief's car) parked in the hotspot, time its presence and check this column.

31) Fire Dept. Drive-through

Check this column every time a fire engine drives through the hotspot without stopping.

32) Parked Ambulance

If you see an ambulance parked in the hotspot, time its presence and check this column.

33) Ambulance Drive-through

Check this column every time an ambulance drives through the hotspot without stopping.

Since Fire often sends a Rescue truck on emergency ambulance calls, please code both the ambulance and the fire engine, even if

they're at the same place.

34) Observer Involvement:

This column indicates that the observer was involved. It should be checked on the same line with the activity in which the observer was involved. (For example, if the police have verbal citizen contact with you, record the time and check both the "Verbal citizen contact" column and the "Observer Inv." column on the same line.

NOTE: There should be only one check per line, with a couple of exceptions:

a) If the first activity by an official presence is a foot-based activity, then you must check either the "arrived in squad" or the "arrived on foot" column as well as the appropriate activity column.

b) If you are involved in an activity, you must check both the appropriate activity column and the "Observer Inv." column.

CODE BOOK: DISORDER

1) **Date:** The date of the observation in 6-digit code, month-day-year, (February 21, 1989, would be written 02-21-89 or 02/21/89).

2) **Hotspot #:** This number should be filled in when you pick up the sheets. If it is not, see a supervisor before you leave to make sure the maps you have are the appropriate ones.

3) **Observer Code:** Copy the number from the "Observer Sequence Code" line on the corresponding "Police Presence" sheet. Include both your three-digit observer number and the four digit sequence code which has been filled in on the "Presence" sheet.

5) **Event Begins**

This column records the time you become aware of an instance of crime or "disorder" commensurate with one of the categories below. Write the time you arrived in this column if the activity was going on when your observation period began.

6) **Event Ends**

This column records the time that the incident under observation reaches a definite conclusion. Write the time your observation period ends in this column if the activity is still going on when your observation period concludes.

*** NOTE: FOR DATA ENTRY PURPOSES, THERE CAN BE A MAXIMUM OF THREE CHECKS PER LINE ON THE DISORDER CODE SHEET.**

7) **Instantaneous Event**

Check this column only if the observed activity is less than a minute in duration. A single loud shout ("Verbal Disorder," #10 below) would merit a check in this column as well as in column #10; a motor vehicle driving through the area with its stereo blasting away would receive a check in this column and a check in "Loud Noise/Music" (#20 below). In these cases, the "Event Ends" time will be the same as the "Event Begins" time. The times must be entered in both columns.

8) **One Person**

Check this column if a single person is responsible for the crime and disorder you observe (i.e., a single drunk staggering around, or sprawled on the sidewalk).

9) **Two or More People**

Check this column if more than one person is involved.

10) **Verbal Disorder**

This column is to be checked when you hear instances of loud shouting, whether friendly, bewildered, or otherwise. A drunk

bewailing his or her fate to the four winds, and loud verbal harassment of passing motorists belong in this category.

11) Loud Dispute

Loud threats and loud arguments between and among people should be coded as a "Loud Dispute" rather than a "Verbal Disorder." The level of aggressiveness and the degree of interaction displayed by two or more participants (either directly overheard, or obvious from facial expressions and/or body language) are the distinguishing criteria. For example, a person who follows another down the street at a distance, shouting and making threats from a safe distance and without getting a response (i.e., there appears to be no imminent or even likely confrontation) should be coded for #10, Verbal Disorder. If the other person confronts him, "Verbal Disorder" should be ended, and a new line begun for "Loud Dispute."

12) Drunk or Drugged

Check this column when you observe a person who is:

- disoriented;
- lacking in coordination, i.e., stumbling, staggering, fumbling with car keys, hand-held packages, etc.
- slurring his/her speech, or rambling speech to everyone or no one in particular.

13) Solicitation

This column indicates sexual solicitation only; it should not be checked for solicitation of other products or services.

Style of dress is not a strong indicator of prostitution activity. Minneapolis Police officers who have worked the Vice Squad advise that a female prostitute's attire may range from the garish-makeup-and-miniskirt-with fishnet stockings- stereotype to jogging suits, snowmobile suits, and the "collegiate look."

Female prostitutes often work alone, while male prostitutes work in groups. Characteristic of both groups is a slow aimless walk confined to a limited area.

The male customer approaching a female prostitute generally does not know the woman; the customers of the male prostitutes have a preference for partners they have used before (though some of the verbal exchanges may indicate that "I haven't seen you before" or "I haven't used you before").

Male prostitutes will wave to cars from the corner; female prostitutes can be more aggressive, stepping into the road, sometimes directly into the path of the oncoming car as though they are flagging down help. There will be a short exchange at the car window, and the prostitute either gets into the car or returns to the sidewalk/corner.

It may take some time before you recognize that the man or woman at the corner is soliciting sexual activity. For our purposes, the "Begin" time for this activity is the first minute that you feel sure you know what they are doing.

14) Drug Activity

Drug activity is marked by very similar behavior patterns,

since the "crack houses" of Minneapolis frequently employ lookouts. The lookout, typically a male, may be visible in a window or on a rooftop.

Sometimes the drug sales are blatant, and money and packets can be observed changing hands. At other times, there will be a brief encounter with a contact person (usually from a car driving by), and the car will stop, the buyer enters a nearby house, and returns within two or three minutes, and drives off.

At other times, the transaction will take place on the street: a third person will join the other two (or the buyer alone); the buyer and seller walk together a ways--sometimes around the block--and at some point money changes hands, at another, the drugs. The drug packet may be dropped on the sidewalk by the seller, and picked up by the buyer; shortly after, the two part company.

As with prostitution, the "Begin" time for this activity is the first minute that you feel sure you know what they are doing.

15) Physical Assault

This column applies to instances of pushing and shoving, or outright attack. It may be accompanied by verbal disorder as well (if so, check both columns on the same line). If the assault is accompanied by a robbery of some kind (i.e., taking something from the victim--wallet, purse, bicycle), check the "Other" column (#22) and write "robbery" in the Comments column.

16) Automobile Break-in

Check this column if you observe someone break into an automobile, either by jimmying the locks or by smashing a window. If the person then drives off with the car, write "Possible Auto Theft" in the Comments column.

17) Building Break-in

Check this column if you observe someone forcing open a door or window of a building. Note in the Comments column whether the building is a residential or commercial building. If possible, note the address at which this occurs.

18) Vandalism

Check this column whenever you observe someone damage, deface, or destroy property.

19) Person Down

A person who falls and remains down for longer than 15 seconds is considered a "Person Down." That person may be drunk or ill. If you suspect a heart attack, call 911 for an ambulance. If the weather is very cold, and a person stays down for more than 5 minutes, call 911. If you call 911 to report a life-threatening situation, do not identify yourself as a Hotspot observer. If possible, identify yourself as a passing motorist, and give the report anonymously.

20) Loud Noise/Music

Loud noise can include loud stereos, boom boxes, power

tools, loud revving motors, band practice, etc. Your tolerance level should be consistent among all of the hotspots, regardless of neighborhood composition.

21) Bag Person

The bag person is shabbily dressed, frequently with numerous layers of clothing in all weather, carrying shopping bags or plastic lawn bags full of his/her belongings, often in carts or on bicycles.

22) Other

We can't think of everything. You may see outright instances of pick-pocketing, of murder, rape, and other varied activities that might tempt Minneapolis property owners to move to and/or shop in the suburbs. Some possibilities include flashing, urinating in public, etc. If an activity offends your sensibilities and doesn't fit any of the categories above, check this column and describe the activity in the Comments column.

23) Observer Involved

This column indicates that the observer was involved. It should be checked on the same line with the activity in which the observer was involved. (For example, if there was a woman who approached you with offers of sexual solicitation, record the time and check both the "Solicitation" column and the "Observer Inv." column on the same line.

APPENDIX A

By: Joanne Oreskovich

CODING VINGETTES

How would you code the following ambiguous situations?

1. You are parked in your hotspot, a car pulls into your view and parks, about 5 minutes later a uniformed officer gets out of the car and enters the building.
2. About halfway through your observation period, a truck leaves it's parking space and behind it you see an unoccupied squad.
3. A squad is stopped in your view for about 2 minutes, with the engine running. An officer gets out and tickets a car, then gets back in the squad and leaves.
4. A squad pulls into the hotspot and parks for about one minute. Two officers get out of the car, one goes into the building, the other talks with a group of people on the sidewalk. After about five minutes the officer in the building comes out and talks with the same group of people and the other officer for a couple of minutes. The officers then get into the squad and leave.
5. A squad passes through your field of vision and turns the corner. About two minutes later two officers on foot walk from the squad's direction and patrol the streets.
6. A squad cruises a parking lot going very slowly, stoopping occasionally, but never parking.
7. A squad with lights and siren on arrives in your hotspot and talks with/or aids accident victims.
8. A foot patrol officer stops a citizen and frisks and cuffs him over the course of about 5 minutes. About 5 minutes later a squad pulls up with lights flashing and the officer gets out and goes to the officer and detained person. All leave in the squad about one minute later.
9. A van repeatedly drives through the hotspot, on two occasions, it stops and the passenger talks with people on the street.
10. A group of young males are sitting on their porch front playing loud music. Occasionally one enters the apartment, comes back out, while a couple of the others talk, shake hands, cajole with the motorists.

4) Hotspot #

3) Time Out:

2) Time In:

Name:

1) Date:

5) Observer Sequence Code

7) Parallel Code:

6) Position

8) Drive-by:

Official Presence

SQUAD BASED ACTIVITY

Out-Based Activity

Other Official Presence

		Official Presence																									
		ENTERS	LEAVES	Activity Begins	Activity Ends	Unoccupied Squad	Code 3 Drive-by	Drive-Thru	TLE	Accident	Stopped in Hotspot	Arrived in Squad	Arrived on Foot	Enters Building	Verbal Citizen Contact	Physical Citizen Contact	Arrest	Security Guard	Security Vehicle	Parked Fire Dept. Equip.	Fire Dept. Drive-Thru	Parked Ambulance	Ambulance Drive-Thru	Observer Inv.	COMMENTS		
TEXT	EXAMPLE #1	1	7:00	7:14		1						1	X					1									
		2		7:00	7:05	2						2		X				2									
		3		7:10	7:12	3						3						3									
		4		7:12	7:14	4				X	4			X				4									
[REDACTED]																											
#2		6	7:00	7:12		6				X	6							6									
		7		7:01	7:10	7					7			X	X			7									
		8		7:10	7:12	8					8			X				8									
[REDACTED]																											
#3		10	7:00	7:10		10					10		X	X				10									
[REDACTED]																											
#4		12	7:00	7:08		12				X	12							12									
		13		7:02	7:04	13					13				X			13									
		14		7:04	7:07	14					14					X		14									
		15		7:07	7:08	15				X	15							15									
[REDACTED]																											
		17	VINGETTES: (Assume all observations start at 7:00)								17							17									
		18				18					18							18									
#1		19	7:05	7:05		19					19		X		X			19									
[REDACTED]																											
#2		21	7:35	8:10		21					21	X						21									
[REDACTED]																											
#3		23	7:00	7:03		23					23							23									
		24		7:02	7:03	24				X	24							24									
[REDACTED]																											
#4		26	7:00	7:08	7:00	7:01	26				X	26						26									
		27		7:01	7:06	27					27				X			27									
		28		7:01	7:08	28					28					X		28									
		29		7:06	7:08	29					29					X		29									
		30				30					30							30									

Beginning of Shift:

- a) Number of People Out-of-doors _____
 b) Number of Parked Cars _____
 c) Number of Stopped Cars _____

Time: _____

End of Shift:

- a) Number of People Out-of-doors _____
 b) Number of Parked Cars _____
 c) Number of Stopped Cars _____

Time: _____

Name: _____
 1) Date: _____
 2) Time In: _____
 3) Time Out: _____
 4) Hotspot: _____
 5) Observer Sequence Code _____
 6) Drive-by: _____
 7) Parallel Coded: _____

Official Presence				Unit-Based Activity										Other Official Presence										End of Shift:												
ENTERS	LEAVES	Activity Begins	Activity Ends	15	16	17	18	19	20	21	22	23	24	25	26	27	Arrival	Physical Citizen Contact	Verbal Citizen Contact	Enters Building	Foot Patrol	Arrived on Foot	Arrived in Squad	Arrived in Hospital	Stopped in Hospital	Accident	TLE	Drive-Through	Code 3 Drive-by	Unoccupied Squad	28	29	30	a) Number of People Out-of-doors	b) Number of Parked Cars	c) Number of Stopped Cars
11	12	13	14	1	1	1	1	1	1	1	1	1	1	1	1	1																				
1	7:00	7:00		2	2	2	2	2	2	2	2	2	2	2	2	2																				
2	7:02	8:10																																		
36	4	7:00	7:05	4	4	4	4	4	4	4	4	4	4	4	4	4																				
47	6	7:00	7:10	6	6	6	6	6	6	6	6	6	6	6	6	6																				
48	8	7:00	7:11	8	8	8	8	8	8	8	8	8	8	8	8	8																				
9		7:00	7:05	9	9	9	9	9	9	9	9	9	9	9	9	9																				
10		7:00	7:11	10	10	10	10	10	10	10	10	10	10	10	10	10																				
11	7:03	7:11		11	11	11	11	11	11	11	11	11	11	11	11	11																				
12		7:10	7:11	12	12	12	12	12	12	12	12	12	12	12	12	12																				
13				13	13	13	13	13	13	13	13	13	13	13	13	13																				
14				14	14	14	14	14	14	14	14	14	14	14	14	14																				
15				15	15	15	15	15	15	15	15	15	15	15	15	15																				
16				16	16	16	16	16	16	16	16	16	16	16	16	16																				
17				17	17	17	17	17	17	17	17	17	17	17	17	17																				
18				18	18	18	18	18	18	18	18	18	18	18	18	18																				
19				19	19	19	19	19	19	19	19	19	19	19	19	19																				
20				20	20	20	20	20	20	20	20	20	20	20	20	20																				
21				21	21	21	21	21	21	21	21	21	21	21	21	21																				
22				22	22	22	22	22	22	22	22	22	22	22	22	22																				
23				23	23	23	23	23	23	23	23	23	23	23	23	23																				
24				24	24	24	24	24	24	24	24	24	24	24	24	24																				
25				25	25	25	25	25	25	25	25	25	25	25	25	25																				
26				26	26	26	26	26	26	26	26	26	26	26	26	26																				
27				27	27	27	27	27	27	27	27	27	27	27	27	27																				
28				28	28	28	28	28	28	28	28	28	28	28	28	28																				
29				29	29	29	29	29	29	29	29	29	29	29	29	29																				
30				30	30	30	30	30	30	30	30	30	30	30	30	30																				

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